

THE PSYCHOLOGICAL REVIEW.

THE FOURTH INTERNATIONAL CONGRESS OF PSYCHOLOGY.

BY PROFESSOR HOWARD C. WARREN.

Princeton University.

The international gatherings of psychologists, which seem now to have become a permanent institution, originated, it will be remembered, with the Paris Exposition of 1889, when the French authorities arranged for a number of congresses representing various sciences, professions and practical pursuits. The same policy was adopted in connection with the Exposition of 1900, and psychologists were again invited to hold an international congress at Paris. Meanwhile, the idea had taken root, and two other psychological congresses had been held elsewhere—in London (1892) and Munich (1896)—making the present session the fourth of the series. It should not be forgotten, however, that the idea originated in France, and that we are under great obligations to the authorities and psychologists of that country for the practical realization of what was long regarded as merely an impracticable ideal. This obligation is now greatly increased, for the hospitality of the French nation and individuals, and the activity of the committee charged with organizing the Fourth Congress, were chief factors in the success of the meeting.

In attempting an estimate of the value of the Congress and its work, much depends on our conception of the functions of such a gathering. Apart from the reading of papers, the discussion and formal interchange of ideas, and the opportunity

afforded of coming face to face with the personalities behind the writings one has admired or criticised, constitute factors of no small importance in the attraction and helpfulness of these meetings. Indeed, in the minds of some, the order of these elements should be the reverse of that just given. The papers presented (on experimental topics, for example) may or may not furnish contributions of importance to the science. As a rule, their contents, if valuable, are not new; or, if new, are not fully appreciated. The most profitable contributions, with some exceptions, are résumés of results which the authors have recently published, and possibly exploited at their local academies or national associations as well. A new monograph is usually better understood when seen in print than when listened to. But there is not the same facility in discussing the printed as the verbal presentation. A magazine discussion has obvious limitations: it takes time to work out, and where several participate it becomes too complex to follow readily. In a meeting, the more salient points may be easily picked out and discussed by a number of speakers, with profit to themselves and to their audience. For this reason the discussion may be regarded as more important than the papers. The social atmosphere is another element to be considered, and not the least important. This is naturally more varied and more stimulating (if the conditions favor) in the Congress than at local or national meetings. The influence of mere personality should not be underestimated—least of all by psychologists.

Two circumstances militated somewhat against the Paris Congress in these respects. One was the absence of a number of recognized leaders of psychological thought, whose attendance had been counted on. The other was the diversity of language, which proved a greater obstacle than might have been expected.

In view of the relative accessibility of Paris and the added attraction of the Exposition, it was generally believed that the Congress of 1900 would surpass even its immediate predecessor, both in numbers and in the quality of its membership and papers. Moreover the tireless efforts of the Secretary, M. Janet, and the Committee who assisted him, left nothing undone to secure this

result. Any lack that may have been felt in the meetings was certainly not due to any omission on their part. But a number of independent causes seemed to have conspired to deplete the attendance. The hot summer, the crowded state of the city, the high price of living, together with social, personal and other reasons, were responsible for the absence of many prominent foreign psychologists, whose names were actually on the rolls. In this respect the Congress was somewhat disappointing, and probably most so of all to the French psychologists, who were present almost without exception.

The barriers of language, too, were surprisingly strong, considering the growing internationality of psychological literature. A majority of the papers were in French, and even that language was unknown to some of those present, while a large number were obviously unfamiliar with some of the other languages used. This hampered the discussion considerably. It would certainly seem that, with the proceedings limited to the four languages in which most psychological material appears, such a difficulty should not have arisen. However, it must be added, in fairness, that the acoustic properties of the large halls were very poor, and many of the addresses were inaudible except to those quite near the speaker. This made it difficult at best to follow the proceedings carried on in a foreign tongue. But the problem of language remains, and it must be met in connection with future congresses. If the discussion is to attain its end, and if it is to prove really beneficial, it is necessary that every speaker be understood by the greater part of the audience. No international congress can be wholly satisfactory till this condition of affairs is reached. Whether a single medium of communication is better than several, is to-day only a forensic question. The modern tendency is to enlarge the number rather than restrict it. The Russians made an earnest plea at the Congress for the recognition of their tongue. And experience there as elsewhere proved that a speaker was generally more fluent and more easy to follow when using his own language than when attempting to speak in another—that is, of course, to one understanding both. But any multiplication of tongues involves a hardship. At present, 'current' literature on psychology is practically confined

to the four languages used at the Paris Congress; the others, notably Russian, are sealed books to most of us. The most obvious step toward promoting the success of future congresses would be for psychologists to improve their knowledge of these four. And American psychologists have at least as much before them in this respect as any others. The question of Russian will doubtless present itself with growing force in the near future.

This apparently minor matter is dwelt on here because of the notable part it played in the Paris Congress. It was responsible for considerable lack of life in the discussion, besides creating some confusion at times. Few of the papers were discussed, except in the speaker's language, by those accustomed to using it. Lines were unconsciously drawn in this way, and the real benefit of an international gathering was thus to a certain extent lost.

A further difficulty was the absence of printed analyses in the case of many papers; the authors were themselves to blame for this, but the effect was felt largely by the audience. The sectional programs, too, were more in the nature of a Chinese puzzle than a circular intended to impart information. The list of speakers was printed in one place, the titles of communications in another, and the whole so arranged that it was impossible to determine the hour or even the day when a given paper was to be read. But as this was the only shortcoming of the committee it may well be passed over lightly.

Despite these drawbacks the Congress was a notable event and will take a worthy place among its fellows. If it failed to realize every expectation, it certainly did amply repay the attendance. The papers were interesting and the discussions profitable. The topics were drawn from a wide field and covered the latest work and thought in all departments. An admirable proportion was maintained between the different branches, proving the gathering to be quite representative in character. There were over 300 members in attendance, drawn from almost every country. France preponderated in numbers, as was to be expected, while the attendance from Italy and Russia was surprisingly large. The German-speaking coun-

tries were very well represented, and the Anglo-Saxon contingent (largely American) was considerable, though not so numerous as anticipated. The other countries showed well in proportion to their size and distance, even South America, Japan and India sending representatives.

The meetings lasted from August 20th to 25th, with two sessions daily; one was a general session, with select papers from all departments, the other consisted of the sectional meetings. All the sessions were held in the Palais des Congrès, a building within the Exposition grounds, designed especially for the series of congresses held during the year. The card of membership gave admission to the Exposition at all times during the week of the Congress, and afforded reduced rates at many of the 'side-shows.' In this and many other ways attentions of the most generous sort were offered, both by the authorities and by the home members, to their guests.

A reception was given to the members of the Congress by Professor and Mme. Richet, another by Prince Roland Bonaparte, and a third by the Institut Psychique at its headquarters. On two evenings the Committee provided complimentary tickets to theatrical performances in the Exposition. On Thursday a visit was made to Dr. Sollier's Sanatorium, at Boulogne-sur-Seine; and on the 26th Dr. Toulouse invited the Congress to visit the Asylum at Villejuif, and, after showing the guests through the grounds and buildings, gave a successful demonstration of various types of insanity, by practical examples chosen among his patients.

The subscription banquet, Saturday evening, followed the last of the meetings, and was a very pleasant affair. It was held on the first landing of the Eiffel Tower, where the delightful outlook and cool air assisted in making it a fitting termination of the week's labors. The usual complimentary speeches were made by Messrs. Ribot, Ebbinghaus, Sergi, Janet, Lemon, Prince Tarkhanoff and others.

As already said, all sides of psychological thought and investigation were well represented at the Congress. No single subject formed the 'key-note' of the meeting. Undoubtedly the greatest emphasis was laid on problems of theory and nor-

mal experimental research ; but psychical research was also quite prominent. The theoretic problems of psychology received perhaps most attention, all in all. The Section on Introspective Psychology and its Relation to Philosophy (Section II.), presided over by Professor Séailles (Sorbonne), furnished the largest number of papers, and as a rule these were ably written and worthy of careful consideration. The attendance in this section was greater, on the average, than at any of the others. The historical papers of Professors Ribot and Ebbinghaus were presented in general session.

Experimental research may reasonably claim the next place. In addition to the numerous contributions at the special Section on Experimental Psychology and Psycho-physics (III.), under Professor Binet (Sorbonne), one general session was devoted exclusively to papers of this character. The contributions were numerous and covered a great variety of topics, among which may be noted : psycho-physical method, skin sensations, vision, muscle sense, space perception, association, memory, dreams, fatigue, pain, æsthetics and movement. They reflected a healthy activity in research work all along the line, although no discovery of the first magnitude was brought to light. The reports generally embodied the results of careful investigation ; taken together, they represent, so far as the judgment of more critical examination can be anticipated, a distinct contribution to the archives of experimental psychology. The lack of linguistic unity was especially felt in this section ; it interfered with the flow of discussion at points where criticism and reply would have been most profitable to all concerned. So far as it went, the discussion was lively and stimulating.

The interest exhibited in psychical research and spiritualism deserves mention, whatever attitude be taken towards the subject. At the general session set apart for these topics the attendance was apparently at high-water mark. The papers and remarks of Messrs. Flournoy, Myers and Van Eeden commanded general attention, while the presence of the new 'medium,' Mrs. Thompson, did not fail to attract. The most dramatic feature of the Congress also occurred at this session, when Jagadiska Chandra Chatterji (Benares) made an elo-

quent plea for the study, by Western investigators, of the Hindu psychological methods, which deal largely with telepathic phenomena. It would be unfair, however, to lay too much stress on the momentary popularity of this branch at the Congress. The papers, though excellent, were few in number; while many of those attracted to this meeting were 'lay members,' whose interest in matters psychological appeared to be confined to this particular topic. The Section devoted to Hypnotism, Suggestion and Cognate Subjects (V.), under Dr. Bernheim (Nancy), included only a few papers on psychical research.

The other sections were variously attended; as in the departments already mentioned, the papers showed that a satisfactory course of progress was being maintained in these lines. Sections I., on Psychology in Relation to Anatomy and Physiology, and VII., on Comparative Psychology, met in joint session, under Professor Delage (Sorbonne). Section IV., on Pathological Psychology and Psychiatry, was presided over by Dr. Magnan (Paris Academy of Medicine). Section VI., on Social and Criminal Psychology, under Professor Tarde (Collège de France), was least representative, on account of the special congresses on each of these sciences held in Paris during the summer. The work of these sections was supplemented at two of the general sessions.

To speak here of all the 150 or more papers presented at the six general and twenty sectional meetings is manifestly impossible, and it would be difficult to deal fairly with all the sections in a rapid survey. But some of the papers on theoretic and experimental topics should at least be mentioned.

The opening address, by the President, Professor Ribot (Collège de France), was a well-drawn sketch of the development of psychology in the eleven years since the first Paris Congress. Following this was a paper by Professor Ebbinghaus (Breslau), in which he compared psychology at the present day with the state of the science 100 years ago. Both of these addresses were optimistic in character, and formed a suitable introduction to the work of the Congress.

Among the papers dealing with fundamental problems, the most speculative was that of Dr. Aars (Christiania), entitled

'Sieben Rätsel der Psyche.' He dwelt especially on the inexplicable nature of the relation between past and present in consciousness, the irreducible character of sensation, feeling and conation, the antithesis between the processes of assimilation and comparison, and the problem of the projection of subjective experiences into the outer space-world.

In a paper on the relation between mind and body, Dr. Frésic Pavicié (Zagreb) proposes to regard these two, not as distinct in character, with the Dualists, nor as identical, with the Monists, but as constituting the two extremes of a graduated series of forces, the intermediate terms of which are as yet unknown to us. He instances the animal and vegetable worlds as such a series; among their rudimentary forms, the differences are indistinguishable; in their higher developments they are, nevertheless, so distinct that any connection would be impossible to imagine, had we not the intermediate forms before our eyes.

An interesting application of psychological notions to the natural history of philosophy was made in a paper by Freih. v. Ehrenfels (Prag): *Die biologische Wurzel des Positivismus*. He argues that, from a biological standpoint, the positivistic tendency is better adapted to the proper functioning of the *Erkenntnistrieb*, than the metaphysical; it enables us to systematize our knowledge and purposive activity more easily, since it bases the working of the cognitive instinct, in every case, directly on the category of the particular cognition concerned, instead of regarding the latter as derivative. It has therefore a favorable prospect for ultimate survival. Nevertheless, the tendency to speculation is an instinct firmly embedded in human nature, and apparently too deeply ingrained to be uprooted completely. Even an unfavorable biological instinct may bring to light true knowledge.

Professor Marty (Prag), discussing Resemblance, pointed out two distinct meanings of the term: 1. Things which differ so slightly that they are liable to confusion, are called similar; 2. Things which recall one another on account of their agreement. Negatively, the two meanings may be combined: Resemblance is said to exist between things only where the dif-

ference is not so great that the one can no longer recall the other. One thing may recall another without any liability of confusing them. A lesser difference, however, constitutes a *higher degree* of resemblance, and the highest degree of all is where the difference is so slight that one is extremely liable to be mistaken for the other. Resemblance, in either sense, may occur either through partial identity, or without any such identity, as between two species of the same genus. Finally, resemblance is always a matter of immediate perception.

Mlle. Marie Bœuf (Paris), in a contribution to the psychological theory of time, distinguished between *psychological* time (a quality, the degree-of-presence tone), and *linear* time (a quantity, the length and relative positions of events). "Psychological time rests on an intuition;" that is, "the sensation of time originates in the organism"—it is furnished by the organic sense (*sens interne*); in its most primitive form, it is the consciousness of nervous rhythm. From this standpoint, the unit of time is the unit of concentration; when the nervous rhythm is accelerated, the unit of time becomes, not longer, but more 'saturated,' and *vice versa*. This unit varies from species to species; there is, apparently, a tendency to acceleration, in the course of history. The immediate datum is qualitative. The mental *projection* of events along an *extended* time is a subordinate process. Such localization is always with reference to the psychological present; it is merely a translation of nervous tensions "into feelings which give us the various degrees of presence." "The localization in *linear* time, on the other hand, proceeds by means of reference points outside of the self." The conflict between these two schemes gives rise to illusions of time.

Speaking on the Definition of Perception, Professor Claparède (Geneva) began by calling attention to the different usage between English and French on one side and Germans on the other, in the method of subdividing mental phenomena—the former making the distinction between presentation and representation the test, the latter that between simplicity and complexity. Leaving aside questions concerning the genesis of our belief in the external world, the chief characteristics of per-

ception are: 1. That the sense impressions and associated images involved in perception constitute a mental entity. 2. That this mental entity is perceived as not belonging to the self. But mental phenomena include not only the *perception of spatial objects*, but also *mental operations* performed on sense impressions (comparison, sense of duration, etc.). Should the term *perception* be limited to the former class of facts—in which case we could no longer speak of perceiving a difference, or perceiving time? Or should its scope be enlarged so as not to involve any necessary implication of objectivity? If no satisfactory agreement can be reached on this point, it would at least be useful to know how each particular author employs the term.

In a paper entitled: *Le problème de la conscience dans la psychologie expérimentale*, Dr. Philippe (Sorbonne) calls attention to the growing discord between the reports of the individual consciousness and the result of experimental investigation. The length of reaction-times is grossly misjudged; we are 'conscious' of loss of consciousness when only a loss of memory has occurred, etc. Is the science reaching a point where "consciousness must be denied even the cognition of those modifications which constitute its sole *raison d'être*"? The speaker believes that the difficulty arises rather from the faulty classification of mental phenomena which has been handed down to us, and which attributes, not too much, but rather *wrong characteristics* to consciousness.

Among other papers presented may be mentioned those of Professor H. Bergson (Collège de France), on the consciousness of intellectual effort; Dr. Kreibig (Vienna), on the notion of the 'Sinnestäuschung'; Professor von Tschisch (Jurjeff), on theories of pain; Professor Tisserand (Bourges), comparing the Herbartian with the so-called physiological theory of pleasure; and Dr. Hartenberg (Paris), on the psychology of timidity. Professor Münsterberg (Harvard), who was not down on the program for a paper, also made an address.

To pass on to experimental papers. N. Vaschide (Rumania) reported the results of numerous experiments bearing on the relation between muscular and tactile sensibility. The phe-

nomena investigated were: (1) anatomical and physiological; (2) pathological, and (3) psychological. The general conclusion, which all the results seemed to substantiate, was that the muscle sense is distinct and irreducible; its organ is the muscle itself; and it acts, sensorially and intellectually, like all other senses.

S. Alrutz (Upsala) reported several curious phenomena relating to the temperature senses, distinguishing between warmth, cold, heat, pain, burning and smarting sensations.

Dr. P. Mentz (Leipzig) indicated the results of experiments on the comparative saturation of various parts of the spectrum, by direct comparison, using the Method of the Fourth Proportional, with differences slightly above the threshold. The points of maximal saturation thus obtained coincide with the 'purest colors' obtained with other well-known methods. The number of maximal points is of considerable theoretic interest; here, however, the disturbing effects of suggestion and contrast were felt, as well as an uncertainty arising from the difficulty of determining the *direction* of difference with such slight differences as those used. Apart from this, the position of the maxima along the spectrum was satisfactorily determined. The method, said the speaker, might with advantage be used with the doubtful colors in cases of color-blindness. In the same line, Professor Scripture (Yale) exhibited and demonstrated his new apparatus for testing color-blindness.

A paper by Professor Stratton (California), read in his absence by Dr. Woodworth, reported the determination of the *minimum visibile* by a new method. Lines were placed end to end instead of side by side. The threshold value was found to be about 7" of arc instead of 50"-60", as given by the usual method. These results seem to make it "necessary to modify our conception of the spatial signs here, since the above measurements are considerably less than the distance apart of the sensory elements in the retina, and less than can well be accounted for by the muscular apparatus of the eye."

L. Marillier and Dr. Philippe (Sorbonne) announced a set of new æsthesiometric measurements. These were performed on four subjects, over the entire body, so as to furnish a complete to-

pographical chart of space sensibility. Weber's compasses were used, with points of various forms. The measurements were taken along two vertical lines, one on each side of the body, in front, reaching from the shoulder to the great toe; two corresponding lines on the back; an internal and external line on each arm, and two median vertical lines, back and front. The hands and feet were carefully explored, and in other regions transverse measurements were taken in sufficient number to indicate the entire topography. Incidentally, it was found that with two compass points of different character the subject observed a distinction at distances far below the ordinary threshold; the points were often perceived as qualitatively distinct, even where they were not locally distinguished.

In connection with these researches may be mentioned the paper of Professor Külpe (Würzburg) on the relation of the least perceptible difference to differences above the threshold.

Professor Monroe (Westfield, Mass.) reported experiments proving the existence of olfactory images in dreams. Professor Tarkhanoff (St. Petersburg) described certain hallucinations observed in frogs under the influence of ether.

Mlle. Joteyko (Brussels) presented two papers on the subject of fatigue. Her experiments with the ergograph led to the conclusion that 'the motor centers resist fatigue incomparably better than the terminal organs.' The theory of the central origin of motor exhaustion is therefore not confirmed experimentally; all the researches pointed to the peripheral origin of motor fatigue—through the exhaustion, not of the contractile substance, but of the intra-muscular nerve-endings. In her other paper, Mlle. Joteyko applied these results to a theory of the biological value of fatigue, as a means of defense on the part of the organism. Three modes of defense were noted: immediate (peripheral paralysis), preventive (sense of fatigue), and consequential (habituation, rendering the organism more capable of resisting fatigue).

Professor Sommer (Giessen) demonstrated several new apparatus. He exhibited two pieces for the graphic representation of movements in all three dimensions—one for the finer movements of the finger, the other for movements of the lower limbs.

A third apparatus was designed to measure the pupillary reflex under varying degrees of light stimulation.

In the line of Child Psychology, Professor Netchaëff (St. Petersburg) reported a series of observations on the development of memory among school children. Six hundred and eighty-seven scholars of both sexes, between the ages of nine and eighteen, were examined. The tests included memory for objects, sounds, numbers and words representing various classes of sensations and abstract ideas. A steady growth of memory with years was observed in almost all these respects, which was somewhat checked, however, during the period of puberty. A marked difference was found between the various classes of stimuli, and according to the meaning of the word. The boys showed a relatively better memory for objects and sounds, the girls for numbers and words.

N. Vaschide reported some experiments and observations on the constructive imagination in children. The principal subject was carefully studied from the age of six months to four years. The results obtained from him were supplemented by observations on twelve children of both sexes from three to four years of age.

Professor Bryan (Indiana) reported his experiments upon the recently discovered arithmetical prodigy, and Professor Richet (Paris) exhibited a remarkable musical precocity of the age of four.

Miss Paget and Miss Anstruther-Thomson (London) reported the results of an investigation on the rôle of the motor element in the æsthetics of vision. They found that the æsthetic pleasantness or unpleasantness of visual objects depended not only on the activity of the visual organ and the associated muscular processes, but also on the functions of respiration, circulation, equilibrium and internal motor adjustment. The sensations accompanying these are not the effect of the emotion, but rather its cause and explanation, or possibly they constitute the emotion itself. They accompany the activity of the eye, giving the qualities of linear direction and space relation, and all the rhythmic qualities of visual space generally. The presence of these motor elements is discovered by the examination

of various sorts of sensation, some of which are apparent in the metaphors applied to visual sensations, while many others can only be brought to light by experiment. The speakers presented a *questionnaire* on the topic, by means of which they propose to enlarge the scope of their investigation.

The next Congress was announced to meet at Rome in 1904, under the presidency of Professor Sergi, and an international committee was formed to organize and carry out the arrangements.

HOWARD C. WARREN.

PRINCETON UNIVERSITY.

MENTAL FATIGUE. I.

BY DR. EDWARD THORNDIKE,

Teachers College, Columbia University.

II. MENTAL FATIGUE IN SCHOOL CHILDREN.

It has been generally supposed and by some investigators asserted as proved that the child in school is rendered by the work that he does in a single session less able to do mental work. Having worked say an hour, he can, we are told, do during the next hour less work. Attempts have been made to measure this decrease in ability to do mental work due to the work of an hour or a session. Various methods have been used and various results obtained. Some of these have served as excuses for recommendations of shorter periods, longer pauses, etc., etc. There has been, however, about methods, results and recommendations no decided agreement, and the whole matter of mental fatigue in schools is still a problem.

The present research aims to settle the leading question in this problem, the question, namely, of how much less able to work the child is after having done the work of half or the whole of a school day than he was at the start. The results, which will be presented in detail, are unanimously in favor of the answer, "*He is just exactly as able.*" If these results were legitimately obtained, they prove that the work in the case of the schools tested *did not decrease one jot or tittle the ability of the scholars to do mental work.* The rest of this report will, besides giving these results, explain minutely the methods employed, the conditions of their administration, the methods of computing the results and the bearing of our answer to this fundamental question on the whole school fatigue problem.

There are four principal dangers to be guarded against in attempts to investigate and measure changes in the ability to do mental work. First and most important there is danger lest

you measure not the ability but the willingness to do work. The mere fact that a person does less work may as well mean that he did not want to do more as that he was not able to do more. Secondly, there is danger lest you measure not the decrease due to inability, but the decrease due to inability *minus* an increase due to practice. If the same sort of test, *e. g.*, multiplication, is given in the morning and again at noon, the morning practice may improve the quality of the noon test. Thirdly, there is danger lest emotional excitement interfere. Thus the novelty of a first test might improve or injure the quality of the work done in comparison with that of a second test. Fourthly, there is danger that in very subtle ways by differences in the manner of giving instructions to the pupils the nature of the work may be altered.

To escape from all these dangers the following method was devised: The tests of ability to work were not given by the regular school teachers or as a professed school task, but were given by two persons only, both of them strangers to the school children. Thus all influences of conventional habits of working better at one time than another, of repugnance to school tasks, of ennui, of dislike for the teacher, etc., were eliminated except in so far as such lay in the tests themselves and the mental condition of the pupils. Thus we avoid the first danger and measure not the willingness to work for a teacher at certain times in the day, but something at least very near the actual ability to work.

The influence of practice was avoided by never giving the same sort of test to any child twice. Instead of giving the same lot of children a test in multiplication early and another test late, I gave the same multiplication test to one lot of children early and to another lot late in the session. This of course utterly removes practice from the question, but introduces another disturbing factor, differences in the average ability of the two sets of children. This factor did not, however, disturb anything, for: (1) children were taken from the same school grades, and (2) enough were taken to insure approximately equal ability in the two sets, and (3) several different tests were used, so that the set of children which had one kind of test early in the day had

another kind late in the day, and vice versa. Thus the difference in ability, what little there was, counted as much for the early as for the late side, and its presence could be readily detected. This feature in our method is, so far as I know, new and seems worth adopting, not only because it eliminates the influence of practice, but also because it eliminates the contrary influence of possible ennui at doing the same sort of work a second time.

In order that the third influence mentioned, that of emotional excitement at the first visit of a stranger, might not disturb the results, half of the first visits were made early in the school day and half late, so that this influence, if there be such, would affect early and late tests equally. Finally the instructions were, except in the case of about one-twentieth of the children in one single test, given after a regular model, so that chance changes in them might not work to favor either the early or late tests.

As has been stated, a number of different tests of ability to do mental work were used with each class, so that no peculiarity in the nature of the work might render the results unrepresentative of ability to do mental work in general. As will be later seen the large number of children tested insures that the results are not due to individual eccentricities. I may also add here (some things in previous studies of fatigue make the statement necessary) that no child was excused from the tests except for inability to write, that no papers handed in were discarded in reckoning up results save such as are explicitly mentioned, and that all misapprehensions of the tests are noted in the results or otherwise referred to in the text.¹ The results are from school children in general, not from particular school children picked out for the purpose.

The first kind of work given as a test was multiplication. The scholars were given a certain time in which they were told to do as many as they could of the following examples, which were printed on sheets of paper and passed around. Sometimes sheets containing only three or six examples were used, but there was an equality between tests early and tests late in

¹ Save that in *one early* test, 3 papers in multiplication where the children had *copied off* the examples before doing them were not counted.

the day in this respect. The time given was in general short enough to prevent all but one or two of the very quickest workers from finishing the work. (A full account of the administration of these tests will come later.) The following is a facsimile of the set generally used.

MULTIPLICATION EXAMPLES.

$$\begin{array}{r} 7986 \\ \underline{4523} \end{array}$$

$$\begin{array}{r} 7869 \\ \underline{5324} \end{array}$$

$$\begin{array}{r} 9867 \\ \underline{3425} \end{array}$$

$$\begin{array}{r} 8679 \\ \underline{3542} \end{array}$$

$$\begin{array}{r} 7968 \\ \underline{3254} \end{array}$$

$$\begin{array}{r} 7698 \\ \underline{5423} \end{array}$$

$$\begin{array}{r} 8967 \\ \underline{4532} \end{array}$$

$$\begin{array}{r} 7896 \\ \underline{5243} \end{array}$$

$$\begin{array}{r} 6493 \\ \underline{8786} \end{array}$$

The second piece of work was to mark as many of the misspelled words in the following passage as was possible within a certain time. As in test 1, the time allowed was so short that only the very best pupils finished. No perfect paper was handed in.

MARK EVERY WORD THAT IS NOT SPELLED CORRECTLY.

1. On the 3d of September, 1832, intelligence was brought to the collector of Tinnevely that som wildd eliphants had appeared in the neighborhood. A hunting party was imediately formed, and a large number of nattive hunters were engaged. We left the tents, on horsback, at half-past sevin o'clock in the mornning and rode thre miles to an open spote, flanked on one sid bye Rice-fields, and on the other by a jungle.

2. After waiting som time, Captain B—— and myself walked acros the rice-fields to the shad of a tree. There we herd the trum-pett of an elephant; we reshed acros the rice-fields up to our knes in mud, but all in vaiu, thogh we came upon the trak of one of the animels, and then ran five or six hundredd yards iutoo the jungle.

3. After varius false allarms aud vane endevors to discuvor the obgets of our chace, the colector went into the jungle, and Captain B—— and myself into a bed of the stream' where we had sen the traks; and here it was evedent the elaphents had passed to and fro. Disapointed and impasient, we allmost determened to giv up the chace and go home; but shots fird just before us reanimated us, and we proceded, and found the collector had just fird twice.

4. Of we went throuh forest, over ravin, and through strems, till att last, at the top of the ravine, the eliphants were seen. This was a momant of excitment! We wer all scatered. The collector had taken the midle path; Captain B——, some huntsmen, and myself took to the left; and the other hunters scrambled down that to the rite. At this momunt I did not see anything but after advancing a few yards, the hugh hed ef an elephunt shaking abuve the jungle, withen ten yards of us, burst sudenly upon my view.

5. Captain B—— ande a hunter justt befor me; we al fired at the same moment, and in so dirrect a line that the percussion-cap of my gun hitt the hunter, whome I thought at first I had shoot. This acident, thogh it prouved slight, troubled me a litle. The

grate excitement occasioned by seeing, for the first time, a wild beast at liberty and in a state of nature, produced a sensation of hope and fear that was intense.

In the third test the scholars were required (*a*) to write from memory (after seeing them on a chart for ten seconds) the following figures:

7 5 8 4 2 9 6 1 3 4

and (*b*) to do the same with the similar set

8 2 7 3 6 4 9 5 1 9

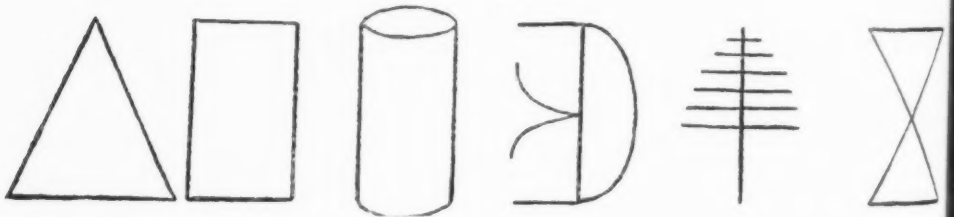
In the fourth test the same thing was done with the following sets of nonsense syllables. These were written in small letters on two charts.

(*a*) ba ni su et ko

(*b*) ig fa tu le ro

In the fifth test the scholars drew from memory the following figures (after ten seconds' exposure on a small chart).

FIG. 1.



The sixth test was like the third, except that the numbers were

(*a*) 4 2 6 9 7 3 8 5 4 6

(*b*) 8 4 2 5 9 7 3 8 9 11

The seventh test was like the third and sixth, except that letters were used instead of figures as follows:

(*a*) P V R E X O A S T I

(*b*) R S K X A N C M L

In test eight the scholars counted the number of dots on a chart which was exposed five seconds.

These tests would seem to be fair measures of the ability of school children from the fifth to the seventh grades to do mental work. The speed and accuracy of the work of the first two will depend, other things being equal, upon the readiness of associations, their accuracy and the amount of attention or concentration bestowed upon the work. The accuracy of the work in the other tests will vary with the intensity of the attention given to the charts, and the success of the pupils in holding the figures, etc. in mind long enough to write them down. The results would not show mental inability so decidedly or delicately as the mental multiplication of fairly large numbers, but for class tests they should answer. In tests one and two less time was given than was necessary to do the examples, so that the amount done as well as the accuracy of the work affords a reliable measure.

Finally, although every precaution possible was taken *not* to have the work done as a regular school lesson, these tests are of the same *kind* of ability as school work requires. If a class does this sort of work as well early as late, it can do its other lessons as well so far as mere ability goes. The mental work was not peculiar or the powers required foreign to ordinary school life.

Before explaining how the quantity and quality of the work were computed it may be well to give a summary of the results obtained with each test.

Test 1. Those who did the multiplication work late in the day did 99.3 per cent. as much and made 3.9 per cent. more mistakes than those who did it early and numbered 64 who misunderstood or grossly failed in the test against 56 among the early ones.

Test 2. The spelling test, which was given early to those who had test 1 late, and vice versa, shows the following results for those who took it late as compared with those who took it early.

Relative percentage of page covered, 99.0 per cent.

Relative number of words marked, 105.0 per cent.

Relative number of words marked improperly, 97.9 per cent.

Thus the decrease in ability shown in test 1 is offset by an equal increase in the case of test 2.

The third test was given to four classes early and four late; the sixth test likewise. Taking both together we find that the pupils who did the work late in the forenoon or late in the afternoon did almost 2 per cent. *better* than those who had the work early. But with tests 4 and 7, where the pupils were reversed, we find the late pupils doing only 98 per cent. as well as the early with test 4 and 99.8 per cent. as well with test 7. So here again the balance is practically equal.

Test 5 was given late to only half the total lot of pupils and the set who took it early were shown by the other tests to be a bit the more intelligent. Here the late pupils did only 94.6 per cent. as well. That this was wholly due to a difference in average ability is also witnessed by the fact that when 75 per cent. of these scholars were given test 8 (those who had 5 early taking 8 late, and vice versa) the scholars taking the test late did much more than five per cent. better. No exact comparison can be made because only six classes out of eight who had test 5 were given test 8.

We have now, after describing the nature of the method in general, given the grounds for our first statement that the child in school could work exactly as well late in the forenoon or afternoon as early in the forenoon. It now becomes our duty to show that these percentages are the legitimate outcome of the particular data and that the tests were so administered that the particular data are reliable.

The particular data are of course the papers handed in by the children. In test 1 we have papers with more or less multiplication work on them. We have to keep account of the amount of work done, the number of mistakes made and of any special blunders. In reckoning the amount of work I took as a unit one of the four products which, when added together, give the answer. Thus a pupil doing one example was scored 4, one doing two examples was scored 8, one doing two examples and two lines of another was scored 10. This is obviously not accurate as it disregards the work of addition, but with over 375

students in both early and late tests it is impartial and is more accurate than a scoring by finished examples. And if at any time the method is questioned one can readily turn the results into double records of the number of examples finished and the number of lines done besides. I am sure no change would be found in the relations of the results of the early test to those of the late. In reckoning the number of mistakes I reckoned in the case of the finished examples only mistakes in the answer, and in the case of unfinished examples all mistakes in the work. Each wrong or omitted figure was counted one mistake, save that where two wrong figures might have been due to a mistake of one or two in the work (*e. g.*, 80 for 79, 61 for 59) I called them one mistake. Special blunders occurred to a considerable extent in the lowest grades tested, where the pupils had evidently not thoroughly mastered multiplication. Some added, some subtracted, some multiplied the multiplicand by only a single figure of the multiplier, some started to add and then multiplied, some started to subtract and then multiplied. Record was kept of all such, and the 58 and 64 in the summary of results refer to these. One sort of mistake might be recorded by itself, but was made to equal five regular mistakes by following the regular method. I refer to the omission of the last partial product, an omission found in very good papers both early and late. As it was not a miscomprehension but just a grave blunder I in every case reckoned the example as finished with five mistakes. The records of the individual pupils were added together by classes. We have thus the following table (I.).

Now before comparing the totals it is necessary to reduce the early and late work to an equivalence in the number of students. In doing this it is not enough to add all the early and all the late pupils and to subtract from the work done and mistakes made by the larger number amounts proportional to the excess in the number of scholars, for 5th, 6th and 7th grade students are not on an equality in this regard. Consequently we have to treat the work of the students of the different grades separately. The most satisfactory way seemed to be to estimate the work of 37 of the 40 students in J so as to have J compare with V; to estimate the work of 29 of the 32 students of O, of

TABLE I.—MULTIPLICATION.

EARLY; *i. e.*, done between 10 and 40 minutes after school opened in the morning.

Time given for Test.	Class.	Grade.	Number of Scholars.	Amount of work done.	Number of Mistakes.	Number of Scholars who added.	Number of Scholars who subtracted.	Number who started by adding but changed.	Number who started by subtracting but changed.	Number who multiplied by only one fig.
4 min.	O	Cleveland 5th	32	259	91					
5 "	N	" 7th	38	491	132					1
4 "	K	" 6th	42	395	87					
5 "	J	" 7th	40	546	127					
6 "	H	Scranton 5th	44	225	95	5	5			6
" "	I	" "	40	482	111					3
" "	F	" 6th	35	239	78	1	1			4
" "	G	" "	37	460	146					1
" "	A	" 5th	45	214	92	5	5	4	5	
" "	D	" 6th	36	276	76	7		5		
Total = 58										

LATE; *i. e.*, done between 10 and 40 minutes before school closed in the forenoon or afternoon.

Time given for Test.	Class.	Grade.	Number of Scholars.	Amount of work done.	Number of Mistakes.	Number of Scholars who added.	Number of Scholars who subtracted.	Number who started by adding but changed.	Number who started by subtracting but changed.	Number who multiplied by only one fig.
Same as O	T	Cleveland 5th	29	254	79					
" " N	M	" 7th	38	561	98				1	
" " K	L	" 6th	40	386	121					
" " J	V	" 7th	37	622	135					2
" " H	R	Scranton 5th	43	319	138	3		1	1	4
" " "	P	" "	42	210	115	6	2			2
" " "	S	" 6th	32	299	109					5
" " "	Q	" "	37	323	132					
" " "	B	" 5th	42	74	34	1	7	3	6	11
" " "	E	" 6th	37	299	88	5	0	3	1	
Total = 64										

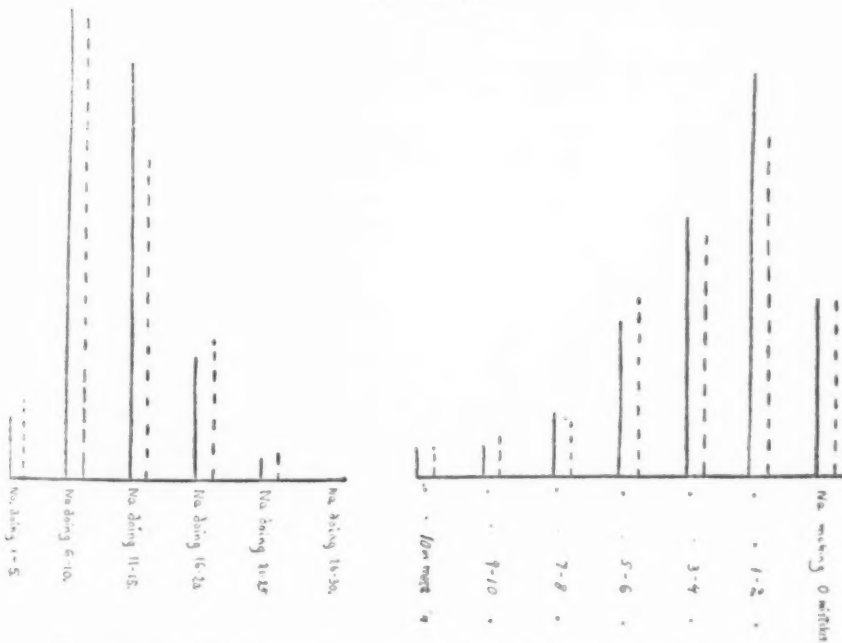
40 of the 42 students of K; to estimate the work of 84 students of the 85 of R and P, of 32 of the 35 students of F, of 42 of the 45 students of A and 36 of the 37 students of E. The reason for not ranking classes A and E with the other Scranton 5th and 6th grades is that they happened to have been already computed. It makes practically no difference. The method of estimating was to deduct in the case of each of the above sets from the amount, mistakes, etc., a percentage equal to the percentage of students deducted. We now have equivalence in the number of students and can add up the work, mistakes, etc., and by doing so we get the following results:

375 early students (estimated from 389) did 3,359 units of work with 1,004 mistakes and 56 very bad papers (*e. g.*, addition instead of multiplication); 375 late students (estimated from 377) did 3,333 units of work with 1,044 mistakes and 64 very bad papers.

From these figures we derive the percentages already given, viz., that those having the test late in the day did 99.3 per cent. as much work, made 103.9 per cent. as many mistakes and numbered besides 64 very bad papers as against 56 in the case of those taking the test early.

Besides this comparison of the total work done I computed the number of those who in the early test did from 1 to 5, 6 to 10, 11 to 15, 16 to 20, 21 to 25, etc., units of work, and similarly of those who took the test late in the day. This was done, of course, in order to detect any phenomena, such as a betterment of the good work offset by a depreciation of the poor work, or

TABLE II.



vice versa, which the mere totals would not necessarily show. A similar computation was made of the number of mistakes. The results follow in Table II. It is evident that the percentages of the totals hold also for the students of different degrees of ability and advancement.

In computing the work in the spelling test we have to note not only the number of words marked, but also the number marked that should not have been and the number of lines covered by the pupil. The last thing is particularly important, as it is better work for a student to have marked 50 words in the first 15 lines than in 20. It shows more care and would require more time of anyone, other things being equal.

In recording the number of lines traversed by the pupil, I took the number from the beginning to the lowest line containing a mark, but did not count the sixth line 'the other by a jungle' nor the eleventh line 'the jungle,' nor the last line 'that was intense.' The incomplete lines beginning 'and we proceeded' and 'yards of us' were however reckoned as full lines. Here and there I found a paper where the child had either marked the places of the letters omitted and the wrong letters or re-spelled the word correctly. These papers (about a dozen in all, I think) I threw out entirely. I am sure that they were not peculiar to either early or late work. It was a blunder not to keep separate records of all such, and if anyone thinks they would alter the results I will go through the 700 and more papers and look them up, but unless some one judges it necessary it seems a mere waste of time. There was one other misconception, a single case, where the scholar did nothing but fill out the three Captain B——'s with Braddock. This I recorded as no lines done, no words marked. By adding up the number of lines done, words marked (whether rightly or wrongly) and words marked wrongly in each class tested we get the results given in Table III.

From these results there was estimated (in the same way as in the multiplication test) the work of 335 scholars who did the work early and 335 scholars who did it late. The reductions were very slight with the exception of E, where the work of 17 pupils was estimated from that of 40. The absence of a score

TABLE III.—SPELLING.

EARLY; *i. e.*, done between 10 and 40 minutes after school opened in the morning.

Time given for Test.	Class.	Grade.	Number of Scholars.	Number of lines done.	Number of words marked.	Number of words marked wrongly.
1 min. 45 sec.	T	Cleveland 5th	29	216	424	2
5 min.	M	" 7th	42	943	2816	35
1 min. 30 sec.	L	" 6th	42	319	756	6
3 min.	V	" 7th	39	493	1518	25
4 min.	R	Scranton 5th	36	555	1035	37
"	P	" "	37	438	697	6
"	S	" 6th	29	529	978	24
"	Q	" "	33	595	1208	25
"	B	" 5th	46	708	1419	13
"	E	" 6th	40	679	1503	18
Total number of Scholars.....			373			

LATE; *i. e.*, done between 10 and 40 minutes before school closed at noon or in the afternoon.

Time given for Test.	Class.	Grade.	Number of Scholars.	Number of lines done.	Number of words marked.	Number of words marked wrongly.
Same as T	O ¹	Cleveland 5th	29	207	507	2
" " M	N	" 7th	37	794	2189	25
" " L	K	" 6th	40	341	848	6
" " V	J	" 7th	40	608	1739	44
" " R	H	Scranton 5th	44	467	834	32
" " "	I	" "	30	463	879	10
" " "	F	" 6th	34	507	1153	33
" " "	G	" "	20	411	898	13
" " "	A	" 5th	45	785	1711	14
" " "	D	" 6th	17	301	720	13
Total number of Scholars.....			336			

of pupils who were present during the multiplication test leaves a chance for an accidental error. If, that is, the 17 were not truly representative of the 37 who did the multiplication, we may have a slight variation due to their peculiarities. That is, the 335 late's *may* here not exactly represent the 375 early's of

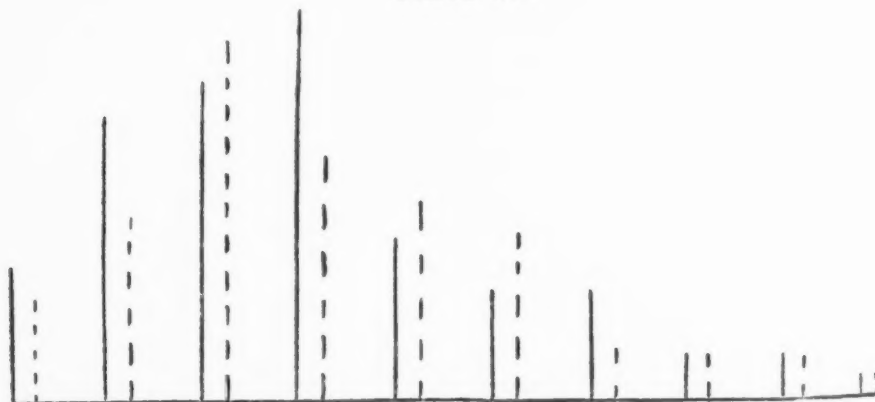
¹By a mistake test O was given 2 min. 5 sec. instead of 1 min. 45, the results being 247 lines, 604 marked, 2 marked wrongly. I have subtracted $\frac{120}{125}$ from each, which may introduce a very slight error, as the work of the last 20 seconds might be at a different rate from that of the rest.

the multiplication test. Of course we have elsewhere not an exact representation of the students who took one test late in those who took the converse test early, because chance absences, etc., make a variation. But it is not worth while to discuss the possible error due to this, as it is in any case so slight. The totals for the spelling test were found to be

Early: 4,917 lines done, 10,482 words marked, 195 marked wrongly.
Late: 4,871 " " 11,385 " " 191 " "

From these totals we get the percentages already given, viz., that the late scholars covered only 99 per cent. as many lines, but marked 105 per cent. as many words and marked only 97.9 per cent. as many wrongly. The number of words marked was also used as a basis of tables showing the number of early and late students who marked from 1 to 10, 11 to 20, 21 to 30, 31 to 40, 41 to 50, etc. As in the case of the multiplication test, this method of treating the results shows that the mere comparison of totals gives a true view of the case. See Table IV.

TABLE IV.



The continuous lines represent the number of early students, the broken lines of late students.

In the third and sixth tests the only record made of each paper's work was of the number of figures correctly given and correctly placed. The general rule was to regard as correct any figure which was in the right place *counting from the be-*

ginning, but it seemed more just and was convenient also to score as correct figures that were *continuously* correctly placed counting from the end. Thus (the model being 7584296134) 758427354 was scored 6, 75878134 was scored 6, 7592481734 was scored 4, while 3758429 would be scored 0. The method is not a complete measure of the quality of work, but it is impartial. The series *b* in test 6 was peculiar in having 11 as the tenth number. This was scored as two one's. The exposures in the case of test 3*a* in one early 5th grade class and one late 5th grade class were five instead of ten seconds. The totals for each class were of course obtained by adding the individual scores. They are presented in Table V.

TABLE V.—FIGURE TEST.

Early.				Late.		
	Class.	Number of Students.	Number of correct figures written (a) and (b) together.	Class.	Number of Students.	Number of correct figures written (a) and (b) together.
Test 3.	T	29	326	O	30	242
	M	42	592	N	36	390
	L	42	453	K	39	410
	V	39	374	J	40	423
	H	44	332	R	43	383
	I	40	311	P	41	419
	F	35	341	S	31	347
	N	37	436	Q	37	464
	Total	308			297	

From the totals for 308 early and 297 late scholars we estimate the totals for 295 early and 295 late scholars of equal school advancement in the same way that we did in the case of the multiplication test, and find that those doing the work early remembered correctly 3,006 figures, while those doing it late remembered 3,060. From these figures we get the relative amount of work of the late pupils, *i. e.*, 102 per cent.

The individual papers written in connection with test 4 (nonsense syllables) were scored in the same way as those of tests 3 and 6. Everything except the correct spelling of a nonsense syllable was marked wrong. To be scored as correct a syllable must be written correctly and either be placed correctly

counting from the beginning, or be one of a *continuous* series of correct syllables counting from the end. The totals by classes are given in Table VI.

The work of test 7 was reckoned up in just the same way as that of tests 3 and 6. The totals by classes are given in Table VI.

TABLE VI.—NONSENSE SYLLABLES AND LETTERS.

Nonsense: Early.			Nonsense: Late.		
Class.	Number of scholars.	Number of correct syllables in (a) and (b).	Class.	Number of Scholars.	Number of correct syllables in (a) and (b).
O	32	220	T	29	188
N	38	282	M	41	318
K	42	304	L	40	262
J	40	297	V	38	296
	152			148	
Letters: Early.			Letters: Late.		
R	37	264	H	44	312
P	38	255	I	31	242
S	31	222	F	35	267
Q	34	344	G	20	174
	140			130	

After estimating for 147 students in the case of the nonsense test and for 130 in the case of the letter test we find the following totals:

Nonsense: Early, 1,081; late, 1,056.

Letters: Early, 997; late, 995.

The late students thus did 98 per cent. as much and 99.8 per cent. as much as the early in these two tests.

As in 3, 4, 6 and 7, so in test 5 (memory of forms), each paper was given a score corresponding to the number of pictures correctly remembered. No attention was paid here to the *position* of the pictures. The criteria of correctness were somewhat complex, certain things being determined upon as the essentials in each form and required to be present, others—*e. g.*, the number of cross lines in the fifth figure—being regarded as unessential. The totals for the different classes tested are given in Table VII.

When we estimate the work of 145 early and 145 late scholars on the basis of these results we find that those having

TABLE VII.—FORM TEST.

Early.			Late.		
Class.	Number of Scholars.	Number of correct figures.	Class.	Number of Scholars.	Number of correct figures.
T	29	130	O	30	112
M	42	182	N	36	154
L	42	183	K	40	169
V	39	168	J	40	167
Totals	152			146	

the test early remembered 628, those having it late 594. We thus get the relative amount of work of the late scholars as 94.6 per cent. of what the early scholars did.

Test 8 (counting dots) was given to only six out of these eight classes. In the case of each student record was kept of whether he counted correctly or not, and if the latter how far he deviated from the correct answer. There were fourteen dots on the chart. Consequently a paper which said '13' would be scored 'wrong, 1'; a paper which said '17' would be scored 'wrong, 3.' The test is such as requires a large number of subjects to make it valuable, and Table VIII., which presents the results obtained, is valuable mainly only because it shows that in the form test the early students were superior in general ability to the late.

TABLE VIII.—COUNTING DOTS.

Early.				Late.			
Class.	Number of Scholars.	Number of wrong answers.	Total extent of deviations.	Class.	Number of Scholars.	Number of wrong answers.	Total extent of deviations.
N	38	11	23	M	41	9	11
K	42	15	28	L	40	4	5
J	40	17	20	V	38	9	16
Totals	120	43	71		119	22	32

As the figures concerned vary so much and so regularly in favor of the *late* scholars we need not estimate carefully the differences, but simply leave the totals as a basis for the reader's judgment. The late have 81 per cent. correct answers to 64 per cent. in the early and do not make nearly so bad mistakes.

In general it may fairly be said of the results presented that they are obtained from the actual work without any complicated processes of allowances, are extremely clear in their meaning, and so calculated as to admit of very slight errors due to absences from class, estimations on per cent. basis, etc. I have gone through all the work raising the classes which had fewer students to an equality with the others by *adding* the proper percentage to their work (instead of subtracting from the work of the classes with more students, as was done in obtaining the results given), and find that it makes no difference in the general meaning of the results and no difference of over 3 per cent. in any single result. It will be remembered that in an early section of this paper I mentioned that in one class a variation in the instruction given may have lessened the work. This was in the case of T. in the early spelling. I accidentally said, "Look at each word and mark it if it is spelled wrongly," instead of saying as usual simply, "Mark this way" (showing them) "every word that isn't spelled correctly." This difference in the instructions given probably made them work a bit more slowly, and consequently a slight addition to the early spelling totals might be made, possibly enough to make $\frac{1}{2}$ of 1 per cent. increase.

The percentages given would seem, on the whole, to be legitimately obtained from the individual papers, and the individual papers would seem to be fair measures of the ability of the children to do mental work. It remains to explain the details of the administration of these tests. The reader will remember that an attempt was made to eliminate the influence of the excitement due to a first visit by making the first visit correspond with an 'early' test in half the grades and with a 'late' test in the other half. This was done in the manner indicated in the following table (IX.), which shows also whether

TABLE IX.

	Class.	First visit.			Second visit.		
		Grade.	Time.	Day of week.	Time.	Day of Week.	Interval of time since first visit.
March 20.	T	Cleve. 5th.	Early A.M.	Monday.	Late A.M.	Monday.	Given same day.
April 10.	V	" 6th.	" "	"	" "	"	" "
April 11.	K	" 7th.	" "	"	" "	"	" "
April 12.	J	" 6th.	Late P.M.	Tuesday.	Early A.M.	Wednesday.	Given the next day.
April 12.	L	" 7th.	" "	"	" "	"	" "
April 13.	M	" 5th.	" "	Wednesday.	" "	Thursday.	" "
	N	" 7th.	" "	"	" "	"	" "
	O	Scr.	Early A.M.	"	" "	"	" "
	A	" 5th.	" "	"	Late P.M.	"	" "
	D	" 6th.	" "	"	" "	"	" "
	B	" 5th.	Late P.M.	"	Early A.M.	"	" "
	E	" 6th.	" "	"	" "	"	" "
	F	" 5th.	Early A.M.	Tuesday.	Late A.M.	Tuesday.	Given a week later.
	H	" 6th.	" "	"	" "	"	" "
	G	" 5th.	" "	"	Early A.M.	"	" "
	q	" 6th.	Early A.M.	"	" "	"	" "
	S	" 5th.	" "	"	Late A.M.	Monday.	Given 6 days later.
	P	" 6th.	" "	"	" "	"	" "
	Q	" 5th.	Late A.M.	"	Early A.M.	"	" "
	R	" 6th.	" "	"	" "	"	" "
		" 5th.	" "	"	" "	"	" "

During last half of March and first half of April.

each late test was late in the forenoon or afternoon session, the day of the week and the interval between tests. It will be noted that six of the twenty early tests were on Monday after the two days' rest.

Care was taken that the late tests should in every case be given after a regular day's work. Where the first visit was made late, the teacher had done her work with the class without any knowledge that there was to be a test of any sort given by anybody. Where the first visit was made early the teacher was kept in ignorance of the day when the second test was to be given and generally of the fact that there was to be any second test. Before giving the late test the experimenter made sure that a regular day's work had been done so far as the teacher's word could make that sure. Miss F. Grace Schaeffer, who conducted the Scranton tests, kept a record in every case of the school program for the day's work before a late test. These records are those of a typical school day in a city grade-school. It seems safe to say that any difference in the ability of scholars to do mental work due to two hours or more of ordinary school work without long rest, or four hours with a rest of one (Scranton) or two (Cleveland) hours inserted, would have been detected by these tests. The tests, then, measured ability to work; the process of computation of totals did not involve any appreciable error; and the work whose influence the difference between early and late results measured was normal work. Such normal work, we have seen, does not decrease the ability to work.

Apparently there is no doubt that the pupils tried as hard in the late as in the early tests, because they did as well, but it may be worth while to describe the means taken to prevent conventional school habits from influencing the work. The means were simply the conduct of the work by Miss Schaeffer (Scranton tests) and myself, the assumption of a human instead of a school-teacher's style of talk about the work, and a plain, matter-of-fact administration of the whole affair.

Some minor explanations are still to be made. In about 90 per cent. of these tests a test of the ability to add was given both

early and late (in order to get data concerning the amount of *unwillingness* to work caused by repeating a familiar task). The addition test took five minutes ordinarily. That made the total actual *working time* of the student during a test from eight to twelve minutes, seven minutes less than the total time taken up with the test. It may be objected that here the pupils have a resting time which would sufficiently prepare them for the short spurt of work. But this objection would be foolish, for they were concentrating their attention on the explanations given by the experimenter, or writing the answers to tests 3, 4, 5, 6, 7 and 8, or waiting attentively for the signal to start, during this time. They were, I should unhesitatingly say, far busier during these seven minutes than during the average recitation in school.

It may also be objected that our tests measuring the mental work done in from $1\frac{3}{4}$ to 6 minutes after a previous five minutes' work at addition do not measure real ability, but only ability to spurt, which it may be claimed is a different thing. But if the pupil can do as much in the last five of ten minutes' work at the end of school as he can in the last five of ten minutes' work at the start, and if in tests 3, 4, 5, 6, 7, 8, which came after the ten minutes, he does as well late as early, there would seem to be no reason why he should suddenly fall off in ability at the 13th or 14th minute. Nobody has ever given any evidence to show that he would. If the mental work done by school children really decreases their ability to work, such decrease ought to appear, at least to some extent, in the work done in these short-time periods. Long periods of work are very objectionable as tests of mental ability in school children, because with them you can not easily get rid of the factors of ennui, unwillingness to work, association in thought with a lesson given by a teacher, etc. I fancy that nobody save one that is obsessed by the idea that school children work too much, will think that our tests involving 15 minutes listening, adding, etc., were too short to feel the influences of mental fatigue. The fatigue must be a very mysterious thing which lets a child work 15 minutes just as well as ever and then suddenly descends upon him. Moreover, I might justly insist that, as the children had been

continuously at work for a half-hour or more before each late test, the time of commencement of the spurt be put 15 minutes before I came into the room. Then my tests should have felt the full force of fatigue. This spurt argument becomes hollow when one realizes that the tests given are not isolated things, but simply 15-minute fragments of the continuous work of the day. The spurt theorist, then, really says only that we have only proved that at any time in the school day the pupil can do as well as at any other. That is all that it is necessary to prove.

Another possible objection might be the limitation of the experiments to the schools in two cities. These cities might be exceptional in requiring less work and giving the pupils an easier day's work in general. Doubtless there are many schools where the pupils do more work in a day. The Cleveland and Scranton schools, however, probably do just as much work as the *average* city schools and very likely more, and our problem concerned the fatiguing influence of school work in general.

A fair claim then to make on the basis of the results obtained is that a regular day's work in the grammar school does not decrease the ability of the child to do mental work. I do not mean that investigation with different tests, longer tests, different schools and different ages should not be carried on with the expectation of modifying this claim. They should be. But until this is done it seems unwise to deny the claim from purely *a priori* reasons. The results obtained by previous observers we noticed at the start as conflicting one with another. In a later paper it is hoped to explain, at least to a certain extent, the factors causing these varying results, especially where they contradict those here presented. This task demands a separate treatment. For the present it will be enough to say that many of the methods (*e. g.*, Griesbach's) did not measure fatigue at all and others measured the unwillingness rather than the inability to work. So we may now turn to take a look at the bearing of our results upon the *practical* problem of school fatigue, first asking the reader to imagine from now on that he agrees with our claim, that the work done in the school session does not decrease at all the ability of the child to do mental work, that he can do as much and do it as well the last hour as the first.

This does not in any sense abolish the practical problem of school fatigue and over-pressure. The fact that the children *can* work as well does not at all mean that they *do* work as well or that measures should not be taken for their relief. It does mean that the argument for shorter hours and longer pauses, so far as *based on alleged incompetency to work under present systems*, was a false argument and its measures for relief ill-considered. We have one clear fact—that children in school ordinarily *do* not do as well in the latter part of the session. The present study has shown one more fact, that this is *not* because they are really any less able to work. To remedy the trouble by decreasing the length of hours, sessions, etc., or by giving easier lessons, would seem then to be an expensive, indirect and possibly unavailing method, for the trouble is not with their *ability*, but their desires, interests and distractions. It is not loss of mental energy, but thoughts of whispering, desires to look out of the window or at the clock, feelings of repugnance at familiar tasks, etc., which lessen the quantity and injure the quality of school work. Mere rest, mere *ease* of work might not remove these and would be at all events a clumsy remedy. The more appropriate remedy would be not to give the student less to do, but to make it *worth while* for him to work, to make the work *interesting*. Many schools in fact would find their scholars doing better work at the end of the day if they gave them *more* work, for in many classes the pupils are bored by the triviality and repetitiousness of the instruction.

No teacher then should think when she sees the scholars' work fall off each hour, "Well, it isn't my fault. If the superintendent will disregard all the laws of hygiene and insist on long work periods and short pauses, the children's ability to work will decrease just as surely as water runs down hill. They can't do but about half as much, and I should just be assisting in their mental break-down if I expected it. The laws of mental fatigue are to blame, not I."

No superintendent should think that by modelling programs after some German authority he will eliminate all of the difficulty. The mental work done is the resultant of two factors, the mental one of ability and the semi-moral one of interest,

willingness, zeal. The first we have shown to be present for the time required without any new program; the latter depends mainly not on programs or hours or rest, but on the material used for study, the methods of teaching and the personality of the teacher. There are, one might say broadly, three main factors which prevent mental work: 1st, the feeling of boredom, of repugnance, a sort of mental nausea; 2d, actual pains, headaches, etc.; 3d, real incompetency, shown in lack of associations, painfulness of all thinking, etc. The 2d and 3d are rare in school, but when present should be the charge of a school physician. The first causes, I believe, 95 per cent. of the decrease in mental work during the day in schools, and for it *good teaching is the cure*. The great burden of the child (and of many of us grown children) is not doing things that are *hard*, or that *hurt*, but doing things that are stupid and sickening and without worth to us.

I said that the fact that the children could work as well at the end as at the beginning of the school session did not abolish the problem of over-pressure. It would of course discredit if not disprove the theory that children spend so much mental energy in school work that they become nervously exhausted, in so far as that theory depends on alleged relative incompetency in children after school work. But over-pressure, so far as due to confinement, restraint, compulsion to attend to lessons possessing no zest, worry, chagrin, eye-strain, etc., would still remain.

Moreover, the reader who has in mind the conclusions of a previous article (PSY. REV., Sept., 1900), will realize that the fact that children can work as well at the end of the day as at the beginning may not at all mean that they have not worked hard or even overworked. For we saw there that the danger point might perhaps be reached without any diminution in the power to work being apparent. Personally, however, from observation of school children at work in classes, from the histories of individual cases and from my own experience, I should say that the *amount* of mental work done by pupils in grammar schools is not the cause of one-tenth of the nervous break-downs among children. The worry, misery and strain of ill-directed

effort and stupid lack of mental stimulus and healthy mental life, together with misuse of the sense-organs, seem to me to be the tap-roots of the evil. The chief responsibility for mental fatigue in the schools and for mental exhaustion in scholars falls, I should be inclined to say, not on a Creator who made our minds so that work hurt them; not on the public opinion which demands that children shall do a given amount of work but upon the unwise choice of material for study, the unwise direction of effort, the unwise inhibition of pleasurable activities, the unwise abuse of sense-organs and the unattractiveness of teacher and teaching.

I have to thank Superintendent L. E. Jones, of Cleveland, Ohio, for permission to test the children in that city, and Miss F. Grace Schaeffer, of Western Reserve University, for taking entire charge of the Scranton tests, and for her careful administration of them. To her zeal I owe very much. For the methods used, the calculations of the results and the conclusions reached I am personally responsible.

III. FATIGUE OF SPECIAL FUNCTIONS.

The previous papers have treated the influence of mental work in general on the general ability of the mind to do work. This is the more important practical question, for it is in most situations in life possible for us to vary the sort of work we have to do and so obviate any possible bad results which might come from continued mental exertion along some one line. Theoretically, however, we may expect as much profit from a study of the influence of protracted exercise of a single mental function on the efficiency of that function. We have seen how far and how general mental work unfits the mind to work further. Let us now ask how far any particular sort of work, *e. g.*, adding, or discriminating weights, or lecturing, or composing, unfits a person to go on successfully with that particular sort of work.

It will be remembered that in the experiment quoted above it was found that three hours of the hardest work that could be devised failed to bring about enough decrease in subject T's efficiency in that particular work to outweigh the increase due

to practice. Observation of my own mental work has convinced me that in other single mental functions I can do a considerable amount of work not only without producing general inability, but also without producing any marked decrease in the efficiency of the particular function concerned. Moreover, as in the case of general mental inefficiency, we found that the reasons for a decrease in work seemed to be physical feelings, strengthened impulses to rest, etc., and other definite afferent feelings which served to bring about a conscious or unconscious inhibition of the mental activity, so I find that the reason why I do less work of some special sort after being engaged in it for a long time is not that I am unable, but exactly because I do not feel like it, *i. e.*, because I feel these afferent inhibitory feelings.

It is difficult to secure subjects who can perform the experiments needed to test this theory, that is, who will carry on a certain sort of work to the utmost of their ability, no matter whether they feel like slackening the work or not. However, my friend Dr. R. S. Woodworth kindly undertook a number of such experiments and carried them out with great care. His results seem to me to enforce the conclusion I have just stated. In them one sees no signs of any gradual impairment of the mental functions involved or indeed within the limits of the experiments (in some cases eight hours long), of any impairment at all. These results are given in detail later on.

It would therefore seem wise to accept provisionally the hypothesis that in special mental functions as well as in general mental activity decrease in efficiency is not the direct consequent and proportionate parallel of mental accomplishment without rest, but is rather due to complex causes, chief among which are feelings other than the mere feeling of incompetence.

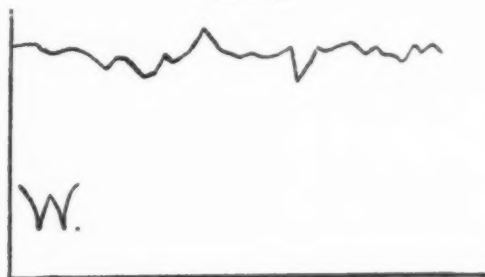
It should be remembered that this does not mean that mental fatigue has not a real existence. Not a jot or tittle of the reality of the fact is here denied; mental inability is a real thing. The view of it taken in this monograph differs from the previous views in ascribing it to different causes.

Experiment 1.

The mental work done was to mark every word containing both *e* and *t* in 151 pages of a book, each page containing about 725 words of text. At the end of each minute a signal was given by a bell and the subject made a mark denoting the point he had reached. The work continued eight hours (from 10:15 A. M. to 6:20 P. M.) with only five interruptions amounting in all to less than nine minutes. The work is hard and requires close attention. It is not unlike a good deal of our ordinary intellectual labors, *e. g.*, reading, correcting proof, noticing details of forms in learning spelling, foreign languages, etc. We can measure any change in its quality by the number of words containing *e* and *t* omitted. Its quantity is of course measured well enough for our purpose by the number of lines gone over in a given time.

It was important of course to have the subject start at a point near perfection, so that practice might not enter largely. The subject had done a very large amount of such work in connection with certain other experiments some months before, and found in a preliminary test of fifty minutes that he was at a dead level (the amounts done in the several ten minutes being 238, 225, 231, 236 and 235 lines).

FIG. A.



The change, or rather lack of change, in the amount done during the eight hours' constant work is shown in Fig. A. The height of the line represents the subject's efficiency as far as concerns amount of work done. It was evident that there was no decrease.

The change in the quality of the work done has not been determined for the whole eight hours. In the first twenty-five minutes' work there were 37 omissions, *i. e.*, 6.075 omissions per hundred lines; while during the last twenty-five minutes' work there were 48 omissions, *i. e.*, 7.92 omissions per hundred lines. Thus there were 30 per cent. more omissions at the end. This does not mean that the quality was 30 per cent. worse, for six omissions in a hundred lines means six omissions out of about 200 cases. If we give the per cents for the number correctly marked we have only nine-tenths of one per cent. difference between the earliest and latest work. It is not possible to tell just how much inferiority in work is represented by an extra omission per hundred lines.

Experiment 3.

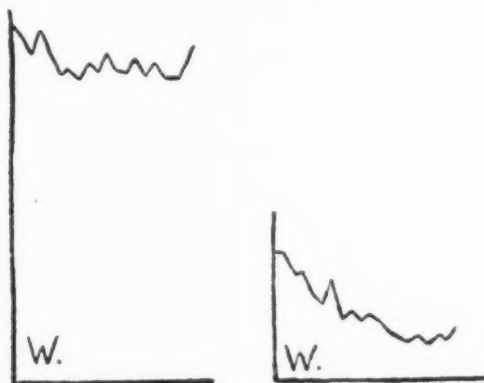
In three hours' work in estimating the areas of small parallelograms of paper the accuracy of W.'s judgment was constant for the first two hours, but fell off 7 per cent. in the last hour.

Experiment 2.

W., who had had some special preliminary drill in memorizing numbers, worked constantly from 3 to 7 P. M., memorizing

FIG. B.

FIG. C.

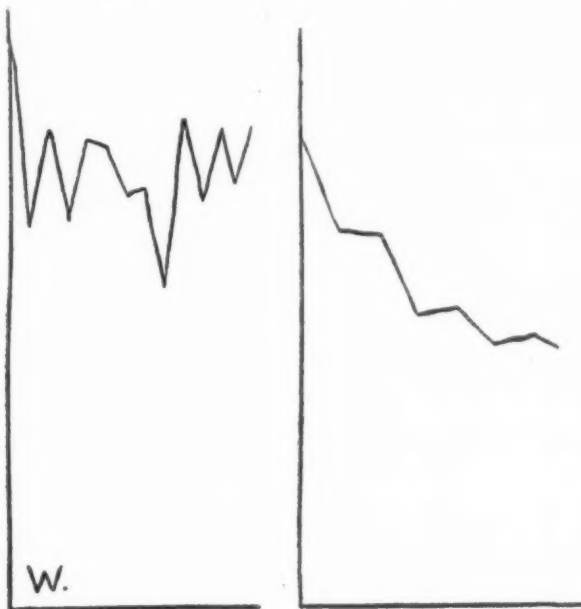


sets of numbers written on a series of cards. Fatigue, if present, did not counterbalance the practice effort shown by Figs.

B and C, which represent the change in the quantity and quality (number of mistakes) of the work. The slight rise in the curves at the end looks like fatigue, but is probably the result of a

FIG. D.

FIG. E.



growing dimness of light noted by the subject at the time of the experiment as troublesome. Apart from this there is no sign of any inability to work.

Experiment 4.

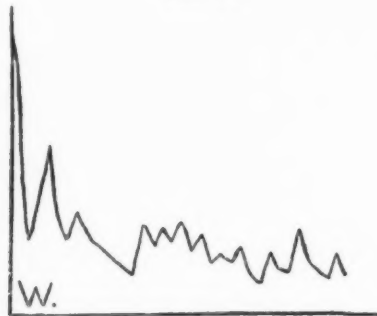
In order to have one experiment which should, as nearly as possible, duplicate an actual piece of mental work such as is frequently done in common life, W. kept an accurate record of the time taken by him in correcting each of seventy examination papers corrected in succession. He worked constantly from 3:30 P. M. to 10 P. M. Of course one would expect considerable assistance to be rendered by practice in this case, but if there were any decrease in ability to work it should certainly appear at the end of the series. Moreover the subject had had a great deal of previous practice in correcting papers. The

six and a half hours of work show no signs of any fatigue effect on the subject's ability. Fig. D represents the change in the amount of time taken per paper (the times for each successive five are represented by the ordinates). Fig. E represents the changes in the amount of time taken per page. Neither of these two ways of reckoning is fully correct; it was easier to look through two pages on one question than one page on each of two questions.

Experiment 5.

Another experiment was taken directly from ordinary life. W. had to go over three hundred and fifty cards on which were written titles of foreign books or articles and decide in each case whether to insert it in a certain bibliography. The work involved careful reading and translation, ability to remember the meaning of technical terms, decision as to the fitness of the article

FIG. F.



and finally decision as to how to classify it. The work took three hours with thirty-five stops of from ten to fifteen seconds (to rest the subject's eyes). The time taken to do each successive ten cards was recorded. No fatigue effect was observable. Fig. F shows the changes of the time in the course of an experiment.

IV. PHYSICAL FATIGUE AS A MEASURE OF MENTAL FATIGUE.

By using dynamometers or ergographs of various sorts we may test an individual's ability to do physical work with his muscles. It has been supposed that the amount of physical

work a person could do and the speed at which the force of a certain movement fatigued (1) are functions of the central nervous system, (2) are dependent on such general factors as are at play in mental work and mental fatigue, and that (3) therefore we may measure mental inability by physical inability. So Kemsies¹ tested school children with Mosso's ergograph, and from differences in the amount of work done at different times in the school day decided that "the school had upon certain scholars an unfavorable influence which we must ascribe to fatigue." A similar method has been used in this country in Chicago to ascertain the loss of power caused by school exercises, and the course of power during the day in certain of the schools has been mapped out.

The first of these suppositions is not decisively proved and may mean various things; the second, so far as it has a definite meaning, is mere presumption; the third, therefore, is a hypothesis to be proved, not a basis for a test of the mental fatigue due to school work. In order that dynamometric tests should have any significance for school work we should have to show how far temporary physical inability signified temporary mental inability. We have already seen how complex a thing temporary mental inability is, how many causes contribute to destroy mental efficiency. We have seen that ability to do mental work is not proportioned to the amount of work done without rest, and that consequently there are two questions, "Does the performance of mental work without rest decrease one's ability to do muscular work and in what proportion?" and "Does *real mental inability* decrease one's ability to do muscular work?"

The first question can be answered by very simple experiments. If we test people with an appropriate dynamometer before and after periods of mental work, we have only to compare the results. By paying due regard to normal variability, practice, physical work, etc., we can estimate the influence, if any, of mental work on muscular ability. The second question is, in the author's mind, too indefinite for treatment as it stands. He would prefer to turn it into a number of specific questions, such as, "Does sleepiness go with a decrease in one's muscular

¹ *Deutsche medicinische Wochenschrift*, July 2, 1896.

ability?" "Does slowness of associative-processes?" "Does inaccuracy?" "Does nervous prostration?" "Does mental irritability?" etc.

The data to be presented here concern the first question only.

The method used was to test the amount of physical work done by various people, before and after periods of mental work, the amount of which was at least approximately known. The apparatus used was Cattell's portable spring-dynamometer, which registers accurately the pressure exerted on a spring. The movements used were contractions of the first, second and third fingers of the left hand. The subjects were college students with some psychological training, an office clerk and myself. The records were taken in the morning after satisfactory sleep and in the evening after a day's class work and study or office work. The dates were so arranged that practice favored early and late tests equally. Variations due to physical work, time of day, barometric pressure, temperature, emotional condition were not excluded. The last three factors probably affected early and late tests equally, if they have any effect. The influence of the time of day could not, for practical reasons, be tested apart from physical and mental work. Very little physical work was done by the subjects of the experiments.

The subject made at each test 100, 200 or 300 contractions, one being made per second, and a rest of a minute intervening between each hundred. The general result of the experiments was to show *that mental work effected no decided decrease of physical power*. In the following figures the broken lines represent the amount of physical force exerted in early tests; the unbroken lines the amount exerted in tests after mental work.

To say that mental work does not necessarily decrease one's power to do physical work does not imply that the latter is independent of mental conditions, permanent or temporary, or that in individual cases whose mental make up was well known dynamometric tests might not be indices of various mental conditions. Among these might be certain of the phenomena of fatigue. What is asserted is that the difference between a mind before and after it has worked for six or eight hours cannot be detected by a record of physical work.



FIG. G. Graphic Representation of Physical Ability Before and After Mental Work.

WELLESLEY COLLEGE PSYCHOLOGICAL
STUDIES.

AN ATTEMPTED EXPERIMENT IN PSYCHOLOGICAL
ÆSTHETICS.

BY PROFESSOR MARY WHITON CALKINS.

With the assistance of

HELEN BUTTRICK AND MABEL M. YOUNG.

The psychologist meets the gravest difficulties in his attempts to bring the genuinely æsthetic experience under experimental conditions, for æsthetic enjoyment cannot be secured by any fixed combination of stimuli, because it is notoriously fleeting, capricious and individual, now surprising one at unexpected turns, and again retreating coyly when most persistently wooed. The simple material, therefore, like straight and curved lines, which lends itself most readily to definite and measurable variation and has formed the material of most experiments of this sort, is peculiarly unlikely to waken the æsthetic thrill. On the other hand, the genuinely beautiful object cannot readily meet experimental requirements. For in spite of theories that there is no distinction between the merely 'pleasant' and the 'beautiful' and that æsthetic enjoyment is simply a peculiarly permanent or an unusually attentive state of pleasure, careful introspection seems clearly to support Kantian theories of æsthetics and to show that 'pleasant' and 'agreeable' differ radically and essentially from 'beautiful.' The truly æsthetic experience is indeed characterized by an almost indescribable merging of self and thing, of ego and alter, of subject and object. It follows that close introspection, the discriminating study of a momentary experience immediately upon its conclusion, is here almost impossible, since the absorption essential to the æsthetic consciousness, is not easily and promptly replaced by the scien-

tific mood. The observer is therefore in danger either of passing over the genuine æsthetic experience or else of accepting as 'æsthetic' the most lifeless and negative moments of pleasure.

For all these reasons, the investigation here described, though it has aimed to study the æsthetic consciousness by the experimental method, can not claim actually to have attained either end. It has tried to avoid the difficulty of the experiments on simple figures, by studying the enjoyment of pictures, but these pictures, though selected for their ability to arouse æsthetic feeling, may have failed of their purpose in many instances. It is indeed impossible to interpret even from their own account of it, the exact nature of the experience of the persons whom we have tested, so that plot or character-interest or some other form of un-æsthetic pleasure may have dominated their enjoyment. Moreover, the very richness and variety of the pictures, which form the material of the study, has diminished their value for strictly experimental purposes. But even when it has not sounded the depths of the genuine æsthetic experience the investigation has been of undoubted interest by bringing into prominence certain conditions of the enjoyment of pictures by young people of various ages.

The subjects of the experiment are three hundred children, equally divided among the kindergarten, the fourth primary and the highest grammar school grades of certain Massachusetts schools; and one hundred and fifty Wellesley College students, of whom one-half are freshmen and one-half seniors.

The immediate object of the experiment is a comparison of the liking of these children with that of the college students for certain typical pictures. The simple method employed is the following: Each person is tested, in a room by himself; three pictures are shown, two by two, in the same order; these two pairs are so arranged that the picture best liked, of the two which are first shown, is again compared with the third picture. Each picture represents a woman's half-figure: Picture I. is a delicately colored lithograph of Prang; it shows the head and shoulders of a pretty pink-cheeked girl who is looking downward. She wears a quaker-like cap and mantle, and the white neckerchief which is open to show her throat is fas-

tened with a bunch of violets. Picture II. is an uncolored photograph of Chantron's 'Souvenir,' shown in the Salon of 1896. The face is slightly turned away and a high light falls on the beautiful features, on the wistful upturned eyes, on the soft hair knotted in the neck, and on the delicate outlines of shoulder, arm and back, from which the garment has fallen away. The right hand, loosely folded over the left, holds a bunch of violets. These two pictures, similarly mounted, are shown side by side, with the question "which do you like better?" (To the older subjects it is explained that this question does not mean "which seems to you the better picture" or "which seems more artistic?") The picture preferred is then combined with Picture III., an *Alinari* photograph of one of Melozzo da Forlì's newly photographed frescoes from the *Capitolo dei Canonici* in St. Peter's, the buoyant figure of the winged, aureoled angel who plays the violin with so serious an expression in her wide-opened eyes.

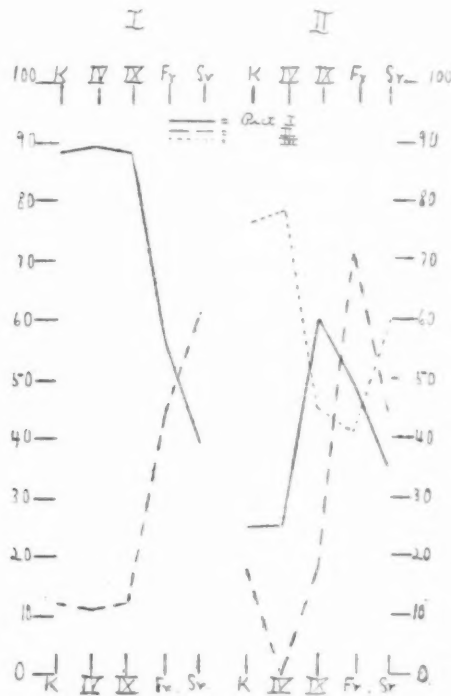
The pictures are, of course, intended to embody in a prominent way certain definite characteristics: the first is colored; the second has the beauty of form and outline, of light and shade; the third is obviously a very suggestive or associative picture, expressing distinct religious conceptions, and symbolizing spiritual experiences, besides inevitably suggesting European galleries and similar works of Italian art to some of those who were the subjects of the experiment. It is impossible, of course, to isolate any of these characteristics; all the pictures have a certain grace of outline and no one of them lacks suggestiveness. But a study of the reasons given for the choices shows that these motives predominate.

The pictures are shown when only the experimenter and the subject of the experiment are present, in order to guard as far as possible against distraction of the attention. When the three choices have been made and entered, the pictures are exhibited again in the same order and the subjects are asked to give reasons for their decisions. These choices furnish the first material for our study. The results are summarized in the table which follows, and are brought out very clearly in the accompanying charts:

TABLE I.—CHOICES.

Age of subjects.	Choice I.		Choice II.		
	Picture I.	II.	I.	II.	III.
	%	%	%	%	%
Kinderg. (90) ¹	87.8 (79) ¹	12.2 (11)	25.3 (20)	18.2 (2)	75.6 (68)
Grade IV. (99)	88.9 (88)	11.1 (11)	25. (22)	—	77.8 (77)
Grade IX. (95)	88.4 (84)	11.6 (11)	59.5 (50)	18.2 (2)	45.3 (43)
Freshmen (70)	55.7 (39)	44.3 (31)	48.7 (19)	71. (22)	41.4 (29)
Seniors (75)	38.7 (29)	61.3 (46)	34.5 (10)	43.5 (20)	60. (45)

The percentages are based on the number representing the possible choices of a given picture. In the first choices, Pictures I. and II. were both shown to each person and either might



have been chosen each time: the percentages are therefore calculated on the number of the persons tested. In the second choice Picture III. was shown to each person and is similarly

¹The parenthesized numerals refer, in the first column to the number of subjects; in the other columns to the number of choices.

treated, but Pictures I. and II. were repeated only when already chosen, and *either*, not *both*, compared with Picture III. The rate of preference for these pictures is based, therefore, on the number of times when they were actually shown in the second test. Picture I., for example, was 79 times preferred to Picture II., by the kindergarten children; in the second test, therefore, it was 79 times shown with Picture III. and 20 times preferred to it; and these 20 choices are 25 per cent. of the 79 possible choices.

In the first choice between the colored picture (I.) and the uncolored figure (II.) nearly nine-tenths of each class of the children, more than one-half the freshmen and nearly two-fifths of the seniors prefer the first, apparently influenced by the color. The children are evidently little affected by the charm of outline or the delicate suggestion of Chattron's picture. The result of this first choice seems therefore to emphasize the prevailing impression that children prefer bright color to any other quality in an object. The study of the second choices does not however substantiate this theory. Precisely the youngest children, those of the kindergarten and the fourth grade, least often choose the colored picture.¹ Children of fourteen, on the other hand, and college freshmen most often persist in their choice of the colored picture. The subtler charm of Picture II. has little effect except upon the college students.² On the other hand, da Forli's angel is, taking all in all, the greatest favorite. So far therefore, as these figures give scope for any conclusion, they point to suggestion as the most common factor of the enjoyment of pictures.

Up to this point, the material for our study, the actual choices of children and of college students, has been of a definite and

¹ The evident interest of the little children in the more novel picture must be taken into account.

² The apparent increase, in the second choice, of the preference of the children for Picture II., is based upon so small a number of cases that it may fairly be disregarded. The most curious feature of the Table is its indication that the liking of the Freshmen for Picture II. is more persistent than that of the Seniors. The number of these choices is not large enough to warrant any dogmatic conclusion, else we should be inclined to interpret the figures as a suggestion that the allegorizing period of the reflective young person's development is the Senior rather than the Freshmen age.

objective sort. In turning now to a consideration of the reasons assigned for these different choices it is necessary to admit at the outset the disadvantages, already mentioned, of the method. The shyness of little children often, undoubtedly, prevented any expression of reasons for their choices, and some of the older children may have hesitated to commit themselves, through a morbid sort of conscientiousness. On the other hand, it is quite likely that college students, and perhaps the older children, occasionally invented very forced reasons from a mistaken sense of scientific duty. Every effort was made to overcome these difficulties; an easy relation with the little children was established, whenever it was possible, before the pictures were shown, and the older subjects of the experiment were carefully told to record only their immediate consciousness. It is nevertheless impossible to assume that our records adequately represent the experience of the persons whom we tested.

The average number of reasons assigned (including those advanced for a third choice) is for the kindergarten children a fraction over three, for children of nine and of fourteen a number between four and five, for college freshmen and seniors five and five and one-half. An interesting general characteristic of the replies is the increase, with advancing years, of the tendency toward the negative reason, that is, the explanation of preference for one picture by dislike of another, as when the colored picture is chosen through distaste for the undraped shoulder and back of Chantron's figure. This class of negative reasons, comprising only one-fiftieth of the explanations given by the very little children, includes one-sixth of the answers of children of fourteen and more than one-fifth of the college reasons—a suggestive commentary on the growth of negative criticism.

The classification of answers to the question, 'Why do you like it?' at first takes account of cases in which the question is met by a 'Don't know' or a 'Can't tell.' Among the little children who could give no reason, the word 'Cause' played a leading rôle in such replies as 'Cause it's prettier' or 'Prettiest, 'cause I like it.' Half the kindergarten children, more than one-fourth of the nine-year-olds, and nearly one-third of the children of fourteen years give no explanation of their

choices, whereas only one-tenth of the college freshmen and an even smaller number of seniors are without this resource. This failure to offer reasons does not, of course, argue against the sincerity of the enjoyment. On the contrary, it is the very nature of the emotional, and especially of the æsthetic, consciousness not to render up an account of itself, for it is an immediate experience, which is followed, not constituted, by the reflective enumeration of its causes. The inability of children to assign reasons for their choices is no indication, therefore, of their inability to feel æsthetic pleasure.

The table which follows offers a classification of the explanations offered, excluding the non-committal answers.

TABLE II.
EXPLANATIONS.¹

Explanation.	Age of Subjects.				
	Kind	IV.	IX.	Freshmen.	Seniors.
Face.	1.3	8.2	8.7	13.8	10.8
Form.	1.3	6	5.9	16.1	11.1
Detail.	75.1	42.5	10.4	4.1	7
Color.	11.7	19.3	25.5	9	9.3
Expression.	.6	5.7	11.5	21.2	17.1
Suggestion.		.6	5.9	13.8	14.4
Relig. Sug'n.	7.5	4.7	4.8	.6	1.2
'Realness'		2.2	6.3	5.1	10.6
Unclassified.	2.	10.4	21.1	16.	18.

The little children most commonly make their choices because of some prominent detail of dress or of background which strikes their fancy. Three-fourths of the positive explanations of the kindergarten children and nearly one-half of those of the nine-year-olds are such as these: 'Pretty suit on and violets'; 'she has curly hair'; 'fiddle'; 'rag on her head'; 'that one has posies'; 'wings on, and playing banjo.' The flowers, sometimes distinguished as 'violets' or 'rosies,' are objects of most eager attention especially on the part of the little children; articles of dress are often named, but now more often by the sophisticated nine-year-olds; the violin and the wings win the attention of the older children chiefly; the hair

¹ This classification of 'reasons' was not pre-arranged, but suggested by the records.

is absorbing, notably to a child who supplies the personal element, and prefers the angel because it has 'curly hair like myself'; the halo is noticed and described as a 'nice head with dots.' What is observable in all this is the narrowness of the child's observation, his frequent inability to appreciate a picture as a whole.

Besides these explanations of preference by singling out some detail of the picture, the kindergarten children have practically only two other reasons for their choices. The most important of these is the liking for color, and the numerical results probably underestimate the importance of this factor, for the admired features of Picture I. were presumably often chosen for their color. Distinctly religious suggestiveness is the remaining expressed explanation of the choice of the youngest children, and is frequently named by older children, while it has no place in the college records. 'Because it's an angel,' 'it's in our church'; 'it's the Christ child's mother'; 'it's a divine picture' are examples of these reasons; and one child says of the angel: 'it's God almighty.'

The nine-year-old children, while still preëminently interested in separate details, name a greater variety of reasons for decision, noticing, for example, face and 'expression' and pose. Children of fourteen, whose fondness for Picture I. has already been remarked, realize their liking for color as the ground of one-fourth of their choices. The pleasure in 'expression' first noticed in the nine-year-olds is more often given as explanation; and under this name are grouped epithets like 'sweet,' 'joyful,' 'thoughtful,' as well as cases of the more specific statement "I like the expression." Still another reason given by these older children for liking a picture is the fact that it is 'real,' or 'human,' or 'alive,' or, as one child puts it, 'natural.' It is interesting to observe that this explanation is never given by the youngest children, who are too inexperienced to make a distinction between real and unreal, but that it is occasionally offered by the nine-year-olds and is most prominent in the records of the college seniors.

College students, as a whole, less often call attention to detail or to color, but more often notice the face and especially

the outline and pose of a figure; in a large number of their choices, they definitely name the expression of the face as its justification; and, finally, they show themselves distinctly susceptible to what has been called the suggestiveness of the pictures. Their records are full of such statements as 'means more,' 'more to it,' 'more soul,' "I like the conception," "it has the right feeling"; and they often name definite associations with pilgrim or puritan, or with 'something of Raphael's that I like very much.' In the same category, but among the negative reasons already discussed, may be included ten instances of objection to a picture for its incongruity with an ideal representation. Thus da Forli's angel is disliked because 'gross for the thing it is meant to represent; not a face for an angel,' while one young person goes into irreverent particulars: "the angel is fat and has a wig, and his halo looks like a door mat." Along with this increase in the influence of general suggestiveness there is a significant reduction in the number of the definitely religious suggestions, which were noticed in the experience of the little children, and this indicates the lessening importance of the religious experience with advancing years and with the multiplication of interests.

Taken together, the explanations of liking for a picture, because of its suggested characteristics, increase with years. Yet it would be unsafe to conclude that little children are unable to appreciate any save the habitual 'religious' suggestions of a picture. Judging from the actual choices, which were unhampered by timidity, by defective introspection and by awkward expression, children are very sensitive to the charm of a pre-eminently suggestive picture. The sweetness of the expression, or the suggestion of victory or peace which a little child finds in a picture is an experience beyond his powers of description, and he is reduced to the reply of the child who said 'I know but I can't tell'; or else, if impressed with the demands of the situation, he selects some easily named detail—like flowers, or wings, or violin—as his 'explanation,' while yet the deeper reason lurks in the fastnesses of his child soul. It would be difficult, indeed, to describe a suggestive picture in more adequate terms than those of one of the older children, a girl of thir-

teen, who said of the angel: "You can think more about it." One should, of course, guard one's self against the common assumption that this avidity for association and suggestion is a mark of æsthetic development. More often interest in the suggestiveness of a picture means the diversion of the attention from the picture itself and the breaking up of that state of absorption which is the condition of purely æsthetic pleasure. Such a comment as the following, by a college senior, shows no trace of æsthetic appreciation: "There is something symbolic, something which goes beyond the mere picture. The wings in the background—express so much." This is a good example of what William James calls 'a glow of spurious sentiment that would have fairly made old Titian sick.'

Among the 'unclassified' answers are a few which deserve a final word of comment. These are, in the first instance, cases of reference to what may be called the technical merit of the picture, to 'clear cut lines,' 'better finish,' 'execution' or 'drawing.' Most of these occur among the older children and the college students, but a few nine-year-olds seem to have been trained to observe these characteristics, and some of their comments, like "stands out more" (of picture II.) and 'not finished in appearance' (of Picture III.) show a genuine observation which far surpasses the vague indefiniteness of such replies as 'more artistic' or 'better art' which are found among the records of the older subjects of the experiment.

The distinctions between the freshman and the senior records are apparent in the table. The differences are too unimportant and the figures too few to demand comment: the greater sensitiveness of the seniors to the suggestion of a picture and their livelier pleasure in its naturalness or realness are the only noticeable distinctions. A very careful comparison of the girls' reasons with those of the boys' gives no support to the theory of a fundamental psychic difference between masculine and feminine point of view. Only two differences are characteristic of all three ages: the girls more often notice the face and the expression in the picture. This distinction seems to be most simply explained by the fact that the less active life of the girl, even in her earliest years, has fostered her interest in dolls and

indoor-plays and picture books, and so has trained her to observe faces and expressions.

The comparison of reasons no longer on the basis of the different ages, but on that of the different pictures, justifies the selection of these particular pictures for the study, since it shows that Picture I. is most often chosen through liking for the color, that Picture II., more frequently than the rest, is preferred for the attitude and outline, the expression and the face; and that Picture III., setting aside the reasons of 'detail' which are given by the children in their frequent selection of it, is most often chosen for its suggestiveness.

The table follows:

TABLE III.
EXPLANATIONS.¹

Explanation.	With Pictures.			Explanation.	With Pictures.		
	I.	II.	III.		I.	II.	III.
Face.	8.3	14.3	9.7	Suggestion.	4.6	7.9	13.7
Form.	7.8	11.1	7.9	Relig. Sug.	—	.3	7.7
Detail.	24	3.6	27.2	'Realness.'	5	10.4	5.1
Color.	28	11.1	2.8	Miscel.	11.1	19.4	14.8
Expression.	10.8	21.5	10.6				

The more general results may be summarized, even at some risk of repetition, lest they disappear in the thicket of percentages. Chief among them is the conclusion that it is impossible to draw absolute distinctions between child and adult, so as to say definitely of this or that picture, "no child will like it," and of this or that motive "no child will be susceptible to it." On the other hand, leaving out of account the preferences of the kindergarten children, there is no type of choice which is not represented among children as among adults; and the common opinion that children, especially little children, are sure to prefer a colored to an uncolored picture is sharply contradicted by our records which show that children are far more impressed by what is called the 'suggestion' of a picture, when this is

¹ The figures suggest a comment on the fact that Picture I. is chosen nearly as often for some detail as for color. As has already been suggested, this special feature of the picture may have been liked for its color; and furthermore, the little children who so often chose Picture I. were frequently too shy to give a reason, although they were very likely influenced by the color.

not of the very subtle sort. What distinguishes the æsthetic judgment of children from that of adults is rather the habit of dwelling on the parts to the neglect of the whole of a composition, and the tendency to fix the attention on the details of figure or of background, and to enjoy a picture through interest in its unessential features. One practical outcome of this study is therefore the suggestion that training in art should aim directly to widen the scope of attention and to stimulate the capacity, never attained by the uncultured person, of seeing parts in relation to each other and in subordination to the whole.

AN ILLUSION OF LENGTH.

BY DR. C. E. SEASHORE AND MABEL C. WILLIAMS.

University of Iowa.

Two years ago, while studying the æsthetics of geometrical forms, we required the students to produce 'double squares' by direct eye estimation. The forms were produced by means of an adjustable frame and were to be made twice as long in the horizontal direction as in the vertical. Not only was the illusion of the vertical counteracted, but there appeared to be a tendency to make the horizontal distance too short.¹

We repeated the same test with two hundred school-children,² ten boys and ten girls of each age from six to fifteen inclusive, and found that this illusion of length is much stronger for children than for adults, and that it decreases with the increasing age of the children. Thus, the children of six made the double square 28% too short, the 7's 24%, the 8's 25%, the 9's 14%, the 10's 10%, the 11's 11%, the 12's 11%, the 13's 10%, the 14's 8% and the 15's 9% too short. These figures seem fabulous, especially when it is remembered that they represent the amount by which the well-known illusion of the vertical has been outweighed. The illusion of the vertical was measured in the same series and found not to vary noticeably with the age of the children.

Continuing the study of this illusion in persons who knew little or nothing about the nature of the illusion, we made a series of measurements with university students in introductory psychology, before the subject of illusions had been discussed in class. Twenty-nine men and thirty-four women volunteered to be tested. The so-called unconscious method was followed

¹ "Forty-eight made the horizontal line too short by an average of 15 mm. (mean variation, 8 mm.), and fourteen made it too long by an average of 10 mm. (mean variation, 6 mm.). On the whole the horizontal distance was made $4\frac{1}{2}$ per cent. too short." *Univ. of Iowa Stud. in Psych.*, 1899, II., 18.












² *Op. cit.*, p. 33.

in so far as it was possible. Most of the observers, however, surmised that some illusion was involved in the test, and the conscious or subconscious reaction to such suspicion often entered into their judgments. The records of two men were discarded because these men asserted that they had attempted to eliminate the illusion of the vertical. The observers may evidently be divided into two classes, namely, those who introduce some correction for supposed illusions and those who do not. But as such division is relative and cannot be made satisfactorily either upon introspective testimony or upon inspection of the results, we group the records together and state some of the conclusions which may be drawn from the averages of the results.

The figures to be studied were cut out of cardboard and then placed on a neutral background upon a large drawing board, which was so placed that the figures were 50 cm. from the eyes and at right angles to the line of regard. Some of the figures were drawn on the background, as will be explained. Every effort was made to eliminate disturbing influences from color, brightness, limiting lines and spaces, movements in the adjustment of the cards and other known sources of error. The forms of figures may be grouped into five classes, as is shown in the accompanying table. They are: (A) parallelograms, (B) a vertical and a horizontal line, (C) and (E) two horizontal lines of different length, and (D) unequal horizontal distances limited by points.

In the A-series rectangular forms were to be produced as follows: A₁, a double square, double in the horizontal direction; A₂, a double square, double in the vertical direction; A₃, a half square, the standard vertical; and A₄, a square, the standard vertical. The standard for all was the side of a square, 114 mm. A card 114 mm. wide and about 275 mm. long was used, and the observer was required to produce the respective forms by placing a similar card over such portion of it that the remaining part would constitute the required form. The figures in the B-series are parallel to those of the A-series and consist of only two of the adjacent lines from each form of the parallelograms. They were to be produced as follows: B₁, the horizontal line twice as long as the vertical; B₂, the ver-

tical line twice as long as the horizontal; B₃, the horizontal line half as long as the vertical; and B₄, the horizontal line equal to the vertical standard. The variable line was 275 mm. long, and the observer covered such portion of it with a card that the remaining part of it stood in the proper ratio to the standard line. In Case C the two lines lie in the same direction, horizontal, and the measurement was made as in the B-series. The variable line was placed 10 mm. lower than the standard in order to eliminate the motive to connect them. In Case D the standard distance (horizontal) was given by two points. The observer was required to place a third point so far to the right of these that the distance between the second and the third points appeared to be twice as great as the distance between the first and the second. In Case E the figure is the same as in Case C, but the observer was required to select the limiting point before applying the limiting card.

Form.											
Case St.	A ₁ 228	A ₂ 228	A ₃ 57	A ₄ 114	B ₁ 228	B ₂ 228	B ₃ 57	B ₄ 114	C 228	D 228	E 228
	X d	X d	X d	X d	X d	X d	X d	X d	X d	X d	X d
M	222 9	213 10	62 3	115 3	226 7	211 6	64 3	119 2	225 8	227 11	217 7
W	222 5	214 5	63 3	116 3	223 7	216 5	63 2	120 4	226 7	227 9	216 9
Ave.	222 7	214 7	63 3	115 3	222 7	214 5	63 2	120 3	226 7	227 10	217 8
% E.	-3 3	-6 3	+11 5	+1 3	-2 3	-6 3	+11 4	+5 3	-1 3	0 4	-5 4

Form, the shape and the position of the figures and forms.

Case, the designation of each case in this report.

St., the required length of the measured line or distance. The unit of measurement is the millimeter.

X, the average of the results of the estimates: (M) by men, (W) by women, and (Ave.) by all together.

d, the averages of the mean variations.

%E, the differences between the estimate and the standard, stated in percentages of the required lengths. This is the illusion. The mean variation is also reduced to percentages.

The number of trials is two for each person upon each point in Cases A, B and D, and ten in Cases C and E. The double fatigue order was observed.

The final averages of the results may be seen in the accompanying table. Case A₁ represents the original form of the illusion. The double square is made 3% too short. This

indicates that the illusion of length exceeds the illusion of the vertical by that amount, because the two illusions stand in direct opposition in this position of the figure. If only the illusion of the vertical had been present the double square would have been made too long. Case A2 shows the effect of the coöperation of the two illusions. Here the double square is made 6% too short. We have found, in a large number of other experiments, that the two illusions in coöperation produce an illusion equal to the sum of the two illusions acting singly; still that is not always to be expected, because the effect of one illusion partly satisfies the motive of the other. Case A3 was introduced as a check upon the method of making the comparison and the measurement. In form this figure is identical with A2, but it differs from it in size, and was produced by varying the shorter dimension. This half-square was made 11% too wide. In all our experiments it is found that when the vertical distance is varied there is a stronger tendency to correct the known illusion of the vertical than when the horizontal distance is varied. This is because more attention is called to the presence of that illusion by the former method. That law does not apply to the illusion of length, because the observers had no means of knowing that such an illusion existed. As will be shown later, the difference in size does not account for the difference in the illusion for the half-square and the double square. Case A4 contains only the illusion of the vertical. The horizontal distance is made 1% too long, but that is not a representative figure. Some four hundred measurements of other students gave an average of 5% for the illusion of the vertical. It is difficult to say how far this tendency to correct the illusion of the vertical enters into the estimation of the double square in the present experiments. It is probable that it increases the difference between the two illusions in A1, and that it decreases the resultant of the two in A2 and A3.

We conclude from the study of the A-series, then: (1) that the illusion of length obtains for both the horizontal and the vertical positions of the double square, (2) that it is stronger than the illusion of the vertical, and (3) that the tendency to make unconscious corrections for an illusion is greatest when the image of the illusion is clearest in the mind.

The B-series was so arranged as to determine whether the illusion is a linear illusion or whether it is peculiar to the comparison of the two dimensions of rectangular areas. The relations of the dimensions of a parallelogram may be estimated: (1) by comparing a vertical margin with a horizontal margin, (2) by comparing the length of imaginary lines that cross at the center of the figure and (3) by judging by the total impression with reference to the areal content without distinctly selecting representative lines. The first method is the easiest and the most accurate, and was followed by nearly all our observers. When the second method is employed the illusion is increased on account of the increased vagueness of the parts to be compared. The third method is the most variable and probably entails the greatest illusion. Children in their quick and spontaneous judgments follow this method, as a rule, and this accounts partly for the strength of the illusion with them. When the first method is employed we have essentially the same conditions of comparison in the A-series as in the B-series. The average of the results for B₁, B₂ and B₃ are equal respectively to the results of A₁, A₂ and A₃. B₄ shows a normal illusion of the vertical.

From the comparison of the results of these two series of tests, we conclude that the gross illusion of length in surfaces is essentially a linear illusion.

Is this linear illusion contingent upon the difference in the direction of the two lines compared? In Case C both lines are placed in the same direction, and the illusion of the vertical is eliminated. All the results for lines lying in the same direction show that the illusion is present in such lines, but is very much diminished by the elimination of the difference in direction.

Case D was introduced in order to determine whether the illusion of distances in the same direction is contingent upon the presence of lines. The results are affirmative; there appears to be no illusion for the distances between the dots.

Case E was introduced in order to determine whether excessive eye movements constitute one of the motives for the illusion. In Cases A, B and C the comparison is naturally made by bisecting the long line visually, to determine whether one half of

the long line is equal to the length of the short line. When one half of the line is regarded the eye wanders beyond the middle point into the other half, on account of the guiding power of the line and the absence of a definite objective limit. This over-running of the point of regard ought to produce an overestimation of the half that is regarded, and thus an overestimation of the whole line. Case E is like Case C in every respect except that there is an additional motive for excessive eye movements in Case E; the eye is allowed to wander to the right beyond the limit to be selected. The results show that the introduction of this additional motive for excessive eye movements produces a marked increase in the illusion. Since this new eye-movement motive which is present in the bisection of a line is of the same nature as the additional one which increases the illusion in Case E, we conclude that it is one of the primary causes of the illusion of length. We may refer to this as the first motive.¹

Contrast is a second primary motive which may be present and coöperate with the first motive. It is an application of the principle of relativity—a long line is compared with a short line, and, according to this principle, the long line should appear to be longer and the short line shorter than it really is. With young children this contrast is undoubtedly the strongest motive, because children have strong tendencies to overestimate differences. It has been demonstrated that illusions that have physiological causes, *e. g.*, the illusion of the vertical and the Müller-Lyer illusion, do not vary in a marked manner with mental development; but the illusions of judgment, *e. g.*, the illusions of contrast and illusions of time, vary enormously with mental development.² Therefore it is probable that, if both the physiological and the psychical motives were present in those of these tests that were made upon children, the variation with age (mental development) is due chiefly to variation in the second motive, namely, contrast.

¹ Incidentally we have here a crucial test of the eye-movement theory of the Müller-Lyer illusion. Cases C and E may be considered the simplest forms of the Müller-Lyer figure. All the motives demanded by current theories of this illusion, except one, are eliminated in these figures. The eye-movement motive is the only one present, and the illusion is present and varies with this motive.

² Seashore, *op. cit.*, pp. 33-35, 83, 84.

These two motives appear in the comparison of lines that lie in the same plane and direction, but when the two lines lie in different directions or are parts of parallelograms, there appears in addition to these a third primary motive. It is a well-known fact that if a square and a double square appear side by side the latter will appear to be the narrower. This is not due to the contiguity of the two figures, for the dimensions of the double square do not appear to change when the square is removed.¹ Some would ascribe it to the principle of relativity, the two sides of the same figure being compared with each other. But it may be accounted for better by Wundt's theory of eye movements, *i. e.*, there is a stronger tendency to move the eyes in the direction of the longer lines than in the direction of the shorter.² This theory is in harmony with the most acceptable theory of the visual perception of space and is well supported by the fact that the total illusion of length is so much greater for parallelograms and lines at right angles to each other than for the lines that lie in the same plane and direction.³ The results tend to show that this third primary motive, which has no connection with the first eye-movement motive, is, for adults, the strongest motive in the figures that contain it.

To determine the validity of the method employed, we made parallel measurements by the method of selection. The observers were allowed to select the required proportions from series of cards. The results obtained by this method support those that were obtained by the method of production as described above.

We also made a series of measurements by the method of selection to determine whether the ratio of 2 : 1, employed above, is peculiarly favorable for the production of the illusion. The observers were required to estimate the length of cards in terms

¹ The theory that the apparent narrowing of the double square is due to an illusion of perspective is disproved by the fact that the figure does not appear to change when the square with which it has been compared is removed, and by the fact that the length of the figure is overestimated, instead of underestimated.

² Wundt, *Die geometrisch-optischen Täuschungen*, Abh. d. math.-phys. Cl. d. k. Sachs. Ges. d. Wiss., XXIV., 1899, 158.

³ A part of this difference may be accounted for by the fact that the placing of the lines in different directions increases the difficulty of comparing and thus gives fuller sway to the illusory motives that are present.

of their standard width. There appears to be a gradual increase in the illusion from its inception near the square up to the ratio of $2\frac{1}{2} : 1$, which was the largest ratio tried. The illusion is not relatively stronger for the ratio of $2 : 1$ than for the adjacent ratios.

Does this illusion vary systematically with the size of the figures? The averages of the results for four observers who made eighty trials each upon each of four sizes of cards of the A2 type above, by the method of production, are as follows: standard (length in the vertical direction) 76 mm., illusion -7% ; standard 114 mm., illusion -8% ; standard 228 mm., illusion -7% ; and standard 456 mm., illusion -8% . The cards were in all cases 75 cm. from the eyes. There is therefore no constant tendency for the combined illusions to vary with the size of the cards. But it has been held that the illusion of the vertical alone increases with the size of the object. We therefore measured the illusion of the vertical for the corresponding sizes of squares. The same four observers made eighty trials each for each size of card, by the method of production, with the following average results: standard 57 mm., illusion -3% ; standard 114 mm., illusion -2% ; standard 228 mm., illusion -4% ; and standard 456 mm., illusion -3% . Thus there is no constant tendency for the illusion of the vertical to vary with the size of the object, and therefore the same is proved for the illusion of length.

DISCUSSION AND REPORTS.

THE DISSIMILARITY IN FUNCTION OF THE RODS AND THE CONES OF THE RETINA.

(What I have proposed to call the normal night-blindness of the fovea (PSYCH. REV., 1895), in parallelism with what has long been known as the (abnormal) night-blindness which is a symptom of *Retinitis pigmentosa*, a disease of the pigment epithelium, one of whose effects is to prevent the formation of the rod pigment and consequently of that surrogate vision which the normal eye acquires in a faint illumination, has not been admitted by all observers to be an actual phenomenon.) Tschermak and Sherman, in particular, have denied its existence, while that has been as vigorously affirmed by v. Kries and his assistants. (A new series of experiments has now been devoted to the question by v. Kries and Nagel¹ (the latter a dichromate). They point out in the first place that the retinal area tested by other observers was altogether too large; even though it did not exceed the size of the rodless region, it was certainly fully equal to it, and there is no possibility of securing such absolute fixation as would be necessary to confine a retinal image for any appreciable time upon a given exact portion of the retina)[even if it was not for the fact that focal fixation in a faint light is any way an exceedingly difficult matter on account of the fact that we have for practical reasons carefully learned to avoid it]. They also desired to meet the objection of Tschermak to their former work (*Ztsch. f. Psychol.*, XII., 1)—that they had not secured a sufficiently long adaptation, and moreover to determine more carefully than they had done before the exact extent of the non-adaptable region.

(The dichromate, and especially the deuteranope ('green-blind'), have great superiority over the non-defective individual as experimenters in subjects of this nature. A difference in brightness is hard to distinguish with accuracy when it is overlaid with difference of color also. But for the dichromate, who sees the entire warm end of the spectrum in yellow, it is easy to make an equation between red rays and

¹ 'Weitere Mittheilungen über die functionelle Sonderstellung des Netzhautcentrums.' J. von Kries u. W. A. Nagel. *Ztsch. f. Psychol. u. Physiol. d. Sinnesorgane*, XXIII., 161-186. 1900.

green rays (to give them their normal names) such that the two fields compared are absolutely indistinguishable. The reinforced vision of twilight, which exists hardly at all for the extreme ends of the spectrum, will then exhibit itself in an excessive brightening of the 'green' field (the difference may amount to a hundredfold), and its failure in the fovea will be evidenced by the fact that for that area the fields remain alike. Instead of exhibiting the two fields side by side, the authors found it much better to use the now familiar 'spot' method—one field is seen through a small hole in the other, and complete likeness of the two is recognized by the disappearance of the hole. (Acuity of vision is so great in the center that the boundary line does not become absolutely invisible throughout its whole extent as it does in the periphery, but there is, nevertheless, perfect certainty as to the equality of the fields.) The size of the hole was in the first instance sometimes $\frac{1}{4}^\circ$ and sometimes $\frac{1}{2}^\circ$. Tried by this method, and with all imaginable subsidiary precautions, it was found that in the exact center of the retina the Purkinje phenomenon does not occur; when the adapted eye is first opened, the spot (which has first been made equally bright with the surrounding 'red' field for the daylight vision of the observer) is seen to shine out with an intense brightness, but as soon as the (minute, black) fixation-point in its center is secured the difference between spot and field absolutely vanishes. There was also no falling off in the saturation of the 'green' spot when viewed centrally, as there would have been if the achromatic twilight vision had overlaid it.)

Experiments were also made by v. Kries himself (that is, with normal vision) by means of comparing two colorless mixtures of different light-ray composition—that mixture which contains green shines out brilliantly in comparison with the other after adaptation (the extended Purkinje phenomenon), and here also the phenomenon was found to be wholly wanting in the center. The same result was obtained, again, when an equation was established, for v. Kries, between the spectral lights red and green on the one hand, and yellow on the other. In one and all of these methods, it will be noticed, the two things to be compared differed not at all in quality, and there was nothing therefore to confuse the judgment of equal intensity; they are much to be preferred to that of the simple estimation of the relative brightness of two adjacent heterochrome fields, as red and blue. [Blue, it should be remarked in passing, is not the color to be chosen in making these comparisons with red. The 'Purkinje phenomenon' is usually stated to consist in the intensification of blue in a faint light

(all mention being omitted of the fact that the blue becomes less saturated at the same time, and that the real phenomenon is a *whitening* of the blue), but this is surely only an intermediate stage, corresponding to that in which the rods are filled with the visual *yellow*, an absorbent for the blue portion of the spectrum. After adaptation is complete, it is green that has become the most intensified, not blue. The first stage may be called by its historical name, the Purkinje phenomenon; the final stage may better be known (for the sake of distinction) as the *extended Purkinje phenomenon*. The gradual change from one stage to the other is exhibited in a diagram in Tonn's paper (*Ztsch. f. Psychol. u. Physiol. d. Sinnesorgane*, VIII., 280), and the fact that the cause of the change is probably to be found in a mixture of two absorbing media gradually changing in relative amount (the *Sehroth* and the *Sehgelb*) is indicated, for the mathematician, in the fact that the several curves have a nearly common point of intersection. The visual yellow is a less marked stage in the regeneration of the visual purple than in its degeneration, when the substances are studied objectively; it would be interesting to know if there is any difference between the subjective brilliancy of blue, according as it is seen in a condition of semi-adaptation following upon the daylight condition or upon the night condition of the eye. This simple experiment has not, I believe, been tried. This intermediate state of vision might well be called twilight-vision, and to the state of complete adaptation the name night-vision might be applied; our ancestors have already made for us the proverb, 'in the *night*, all cats are gray.' Discussion is so voluminous just now in regard to all these new ideas that it is of extreme importance that a good and sufficient phraseology should be adopted as one goes along.

¶ V. Kries and Nagel also made careful experiments to determine the exact size of the adaptationless area by varying the apparent size of the spot by moving it forwards and backwards; it was readily found that the apparent size at which it just fails to undergo a brightening on its outer border is, for the two eyes of a single individual which were tested, 1.40° or 1.8° horizontally, and 1.35° vertically; they also found that at a slightly greater distance from the center there was a sudden sharp access of brightening. They suggest that this latter distance is that at which the arrangement of one cone in a complete circle of rods begins, and that a few scattered rods may occur up to the region of no adaptation, in spite of the fact that Koster gives the rodless region as 2° in diameter. v. Kries thinks it possible that the rod-pigment when outside of the rods, when just formed out of the substance of the pigment epithelium, may be effective upon the adjoining cones. This

suggestion is very lacking in probability, but it is a satisfaction to find that v. Kries begins to attribute to the rod-pigment its evident rôle of being the source of night-vision; he has been speaking even quite lately as if the rods had no daylight-vision at all, and as if the achromatic vision of the normal periphery were necessarily to be assigned to the cones. But this is to overlook the fact, which stares one in the eyes, that the most striking feature of the whole situation is the coincidence between a gradually on-coming re-inforcement of vision, most effective in *green* light, and an equally gradual regeneration of the *green*-absorbing substance in the rods.)

(But there is a still more marked coincidence between function and apparatus adapted to meeting it, which goes very far toward confirming this view that the visual purple acts as an absorbent medium for an insufficiently strong light. All vertebrates except fishes have a visual purple of exactly the same absorption spectrum; in the eye of the fish the maximum absorption is farther toward the blue (Köttgen and Abelsdorff). The difference is perceptible to the naked eye—the color in fishes is plainly more bluish. Now in what respect do fishes, on the one hand, and mammals, amphibians and birds, on the other, differ as regards their faint-light vision? Plainly in this: the dark recesses that other animals have to enter are the depths of forests, the shadows of overhanging plants and grasses. But fishes find their darkness at great depths in the water, and water has its maximum absorption in the yellow (Spring: *Bul. Acad. roy. de Belg.*, 1896 (3), XXXI., 251). The blue absorption stuff of fishes is, therefore, just what is adapted to the retaining of the yellower light of the depths of the ocean. There is another curious feature in the visual apparatus of certain fishes—the retinal tapetum, a white surface of guanin, in the upper half only of the retina, whose evident function is to give preponderating reflection to the light which comes from regions down below, while the too bright light of the external upper half of the visual field is still moderated by the absorption of the black pigment of epithelium and choroid. This is an instance of an absolutely special provision in the eye of the fish for the darkness of ocean depths which is quite analogous to the distinctive color of their visual purple, and hence lends probability to the view that that too is a differentiation in the interest of adaptation to life under special conditions.)

(It is a very great satisfaction to have established, at last, beyond question, the fact of the failure of night-vision in the fovea (meaning by night-vision that form of vision which is acquired after the rod-

pigment—the visual red or the visual yellow—has had time for regeneration, and which is recognized subjectively by the great change which takes place in the relative brightness of the different portions of the spectrum). Experiments on this subject which are made by estimating the brightness of two fields of different color are not to be compared in value (even if they had not been done with too large a field, and by an inadequate method) with those in which the observer is merely required to detect the disappearance of a spot in a surrounding field with which it is absolutely identical both in brightness and color. This necessary condition can be secured in two ways: (1) By the method of the *achromatic color equation*, as it may be called, for the normal eye; in this method the distracting effect of the color-aspect of the fields to be compared is obviated by making each consist of a pair of complementary colors mixed in the right proportion to produce gray, and therefore indistinguishable for sensation when equally bright, though physically of very different constitution. The field which contains green will be the one to brighten up in a faint light, for twilight-vision (being due, as we may suppose, to the absorption into the rods of green light by means of the visual purple) is predominantly vision for the green portion of the spectrum. (2) The same result, that of offering to the observer a difference in brightness only though the ether-wave constitution of the two fields compared, is very different, and can be had with no difficulty throughout either half of the spectrum in the case of the partially color-blind. It was by this method of the achromatic color-equation that Hering's hypothesis of the specific brightening power of the colors (or Hering's complete color theory, if he had not quickly modified his views—*vide* Tschermak) was overthrown: if twilight vision was a new form of vision, mediated by the visual purple of the rods, and for that reason achromatic, and if its changed brightness values were completely accounted for by the character of the visual purple absorption, then there is no room left for a 'specific brightening power of the colors.' Hering's theory is now, therefore, again in its original extraordinary position, in which spectral red, *c. g.*, which most observers regard as being absolutely saturated, owes all its brightness to a black-white process (whose presence at all is a purely gratuitous assumption), and is not affected in its brightness in any degree by the voluminousness of the photochemical process of an exactly similar sort which underlies the sensation of red.

With this definitive result of v. Kries is removed every difficulty that may have existed in granting (what is a starting point for my theory of color vision) that the rods and cones play a different part in the

retinal economy—viz. (to give it accurate expression) that *the rods are the organs for nothing but achromatic vision, and that color is mediated by the cones only*. This result may be regarded, therefore, from now on as a distinct acquisition to our knowledge of the sense for light and color.)

(This view would have been rendered still more certain, perhaps, if it had turned out that all the cases of congenital total color-blindness show also total blindness in the fovea. The first case of that kind to be discovered (the defect does not exhibit itself without some device for making it apparent, on account of the fact that the patient learns carefully to avoid fixating with a spot in the retina with which he can see nothing) was that of the totally color-blind boy in König's laboratory upon whom I applied (1894) the method¹ of showing that some one of a *group of spots* is sure to disappear if such defect exists, when the effort is made to see them all at once—a method which I had just devised in order to render readily perceptible the total lack of the Purkinje vision in the fovea of the normal eye, which had just unexpectedly revealed itself during some experiment which I was carrying out in a dark room in Professor König's laboratory (PSYCHOL. REV., II., 143). Since then there has been much discussion as to whether this total blindness in the fovea of the achromate is the general rule. Uthoff first announced that in a patient of his it did not exist, but since then he has found (using a modification of my method—the person tested endeavors to hold one spot fixated in the exact center of a ring of others) that this same patient can in fact not see anything with the center of the retina. Hess and Hering, Pflüger of Berne, and v. Hippel, have all contributed cases in which this foveal blindness is lacking (they have not yet however used the last described method). But the extreme interest with which the final settlement of this question was regarded is now set at rest by means of the case of Raehlmann (described in the September number of this REVIEW). This case has been known of, as a matter of fact, for many years—that is, though foveal blindness was not expressly looked for, not having been hitherto suspected, it was known that Frau Professor R. had perfect visual acuity, which is incompatible with the fovea being thrown out of function—but it has hitherto failed, through some accident, to be accorded its proper logical weight in the discussion. What is placed beyond dispute by it is that con-

¹ I had already predicted, as a deduction from my theory, that the vision of the congenitally totally color-blind would be found to be vision with the rods only.

genital achromasy *may be* a defect of the cortical centers; this has long been known to be very frequently the sole cause in cases of acquired achromasy (see Förster's case, among many others, *Arch. f. Ophth.*, 36 (194), and since retina and cortical cells are both essential links in the chain of causation in question, and since it is quite certain that there are separate centers for color-vision, there is every reason to suppose that mal-development, as well as disease, may sometimes cause the latter to fall out of function. But, on the other hand, the undeniable cases in which total lack of color-vision is accompanied by total blindness of the fovea, even though they are not exhaustive of all cases, still point as strongly as before to the cones of the retina as being, in these instances, the source of the defect. What is rendered certain by the case of Raehlmann is that there is nothing forced in the referring of cases of a different sort from these to a different seat of the physiological lesion, namely, the cortex.

There is now, therefore, nothing that stands in the way of believing that: (1) *The rods are the organ for nothing but achromatic vision*; (2) *Color is mediated by the cones only*; as I maintained, following upon Max Schultze and Parinaud, in 1892.

C. LADD FRANKLIN.

THE ILLUSION OF DEFLECTED THREADS.

Professor Pierce has reported in the September number of this REVIEW some new and interesting observations on a thread illusion which I described in May, 1898. He has also presented an explanation of the illusion which he regards as simpler and more direct than that originally offered. One almost despairs of ever finding an explanation of any optical illusion that shall command universal acceptance. But it is doubtless the duty of those who are engaged in the investigation of these illusions to compare notes as often and as fully as possible in order to reach some clearness as to the grounds of their differences, even if they find it impossible to accept each other's positions.

The illusion under discussion has as its essential feature the apparent turning of certain parts of two threads so that these parts do not seem to be continuations of the threads to which they belong, but seem to extend away from the threads into the third dimension. The threads which form this illusion lie at different depths in front of the observer, and the upper one passes over the lower one at an acute angle. The points at which the two threads seem to cross are, under these condi-

tions, different for the two eyes. It is the middle part of each thread, or the part between the two crossing points, that is apparently deflected. In some cases, and it should be noted that it is in some cases only, the image of the middle part of the upper thread fuses, not with the other image from the same thread, but with one of the images from the lower thread. That is, instead of the two images from one thread holding together throughout their whole course, they fuse only outside the monocular crossing points. In the middle parts, that is, between the monocular crossing points, they break away from each other and then they may fuse, as was pointed out in the original article, with images from the other thread, even though that other thread is at a different level. It may also be added that this unusual form of fusion may take place even when the two threads are of different colors, so as to give the images very decidedly different characters.

Now the essential difference between Professor Pierce's explanation and mine is, that I lay greatest stress on the *separation* of an image from its true companion, while Professor Pierce lays greatest emphasis on the *fusion* of the images after separation. He assumes that separation of the images is easily possible, and that the moment a new mode of fusion offers itself as a possibility it may be adopted as easily as any other possible mode of fusion. Professor Pierce finds in the presence of a possible new mode of fusion a sufficient explanation of why images which come from a single thread and have been fused through a part of their course, should in the middle part of their course be separated from each other and fused with images from an entirely different thread. In taking this view he discusses only cases in which the point of fixation is chosen half way between the two threads, thus rendering easy the assumption that the images from both threads were double from the outset. His problem, as he sees it, is therefore merely one of finding possible modes of fusion.

The objections that offer themselves to Professor Pierce's view are as follows: First, the deflection often takes place in only one image of one thread. The first case of the illusion discussed in the original paper¹ (and I have confirmed its validity since by a large number of observations with many subjects) is that in which the point of fixation is on one of the threads (not half way between the two threads). When this point on one thread is fixated, the two images of the fixated thread fuse with each other as usual. One of the images, and usually only one of the images, of the other thread will be deflected. These statements can be verified by using colored threads, so that the different

¹ PSYCHOLOGICAL REVIEW, Vol. V., p. 288.

images can be distinguished. This fact shows clearly that deflection is prior to any recombination, and, so far from being dependent on the recombination, is the source of the conditions which make a new and unusual mode of combination possible.

The second objection is this, the mode of fusion to which Professor Pierce refers in his explanation is not the only mode of fusion possible with the images that are given. There is a perfectly regular mode of fusion between the images belonging to the same thread which would result in a perception of each thread as a continuous whole lying at a different level from the other thread. This usual mode of fusion takes place outside of the monocular crossing points. It takes place there so readily and completely that it is very difficult to perceive the double images. Now if the two modes of fusion which are possible, namely, the usual form of continuous fusion and the one that actually takes place, were both equally possible, and if one is chosen to the exclusion of the other, particularly if that one is, to say the least, unusual; then the explanation is not complete until we have been told why this particular choice is made. The statement that a certain choice is made is merely the descriptive preliminary to the more fundamental question of why it was made. As a mere possibility the chances are no more in favor of one mode of fusion than of the other. But in the illusion in hand complete normal, or usual, fusion never takes place in portions of the thread lying between the monocular crossing points. Something has happened to prevent it. What is that something? Certainly it is not enough to say that another mode of fusion has presented itself as possible. As a matter of fact, another mode of fusion has appeared which is not merely possible, but *necessary*. And this necessity Professor Pierce has not explained.

Finally, to deal with two particular points which Professor Pierce dwells upon with great emphasis. The first is that the apparent deflection appears even when the monocular crossing points are covered up. The conditions that result from this covering up of the monocular crossing points are essentially different from those which appear in the ordinary illusion. In the ordinary case of the illusion there is a mixture of what may be called monocular and binocular modes of interpretation. The result is that we see both the separate threads at different depths and also the oblique, deflected threads. When the monocular crossing points are covered, the middle parts of the threads are cut off from the end parts. The result is that the binocular mode of interpretation is very much weakened. So much weakened, indeed,

that *one can never see both the deflections and the threads in their real positions, but only the deflections*. Now the seeing of the deflections *only*, means that the two images that enter a single eye are treated exactly as if they came from a single plane. They might, for example, have been lines drawn on a single plane card. But this treatment of images which really come from different depths as if they came from a single plane, is what the original article attempted to point out as the essential characteristic of monocular vision in general, and of this part of the threads in particular. In covering up the monocular crossing points and isolating the middle sectors of the threads, Professor Pierce has, therefore, shown what becomes of the illusion when the binocular factors are cut out of the original conditions and the monocular factors alone are left. He has thus contributed a very interesting and striking confirmation of the general position that images which fall in a single eye are treated exactly as if they came from a single plane. What Professor Pierce has failed to see is, that the particular case of the illusion which he emphasizes is a particular and limited case, not the original form.

The second point with which I wish to deal is one on which I have no doubt that Professor Pierce will be in fullest agreement with me, but one which it is perhaps well to bring out for the sake of the general discussion of the whole subject of illusions. Professor Pierce writes in one place, 'nothing but the universal laws of tri-dimensional vision are here in operation.' And again he writes, "In view of these various considerations I cannot refrain from the conclusion that Professor Judd's illusion presents no new visual principle * * * I am convinced that we have before us in this illusion only a particularly interesting case of what is eternally happening whenever we open our eyes to see."

The article describing the illusion was very misleading if it gave the impression that its author supposed that the facts presented led to the discovery of any new principle. I take it that the value of any illusion is to be found in the fact that the old 'universal laws of tri-dimensional vision' are absolutely reliable and unchangeable. What we try to do is to see how unusual appearances can be reduced to these same universal principles. Now in the illusion under discussion the appearance is unusual, while the principle of vision may be safely assumed to be the same as usual. The unusual appearance must be due to an unusual combination of conditions. That unusual combination of conditions is looked for in the fact that a part of the field is seen chiefly as one eye would look at it without the help of

the other, while the rest of the field is seen by both eyes in joint action, just as it usually is seen under ordinary conditions.

When Professor Pierce says of the result that it is 'what is eternally happening whenever we open our eyes to see,' I should be in most hearty accord with his statement if it referred to only the general principles of vision involved. Certainly visual perception must here, as elsewhere, follow the long and fully established mode of this form of mental activity. But if Professor Pierce means that under the ordinary conditions of visual perception, that is, under the conditions which are constantly recurring when we open our eyes to see, images from one object may fuse for a certain distance and then suddenly break off to fuse with other images in the field of vision and finally come back to the original mode of fusion—if, in other words, he means that the conditions of this particular thread deflections are everywhere common, then I shall have to take exception to his conclusions. The distinction between uniformity of principles and invariability of typical conditions is worth keeping clearly in mind—for, if I mistake not, the value of the whole study of illusions depends on this distinction.

CHARLES H. JUDD.

NEW YORK UNIVERSITY.

THE SPACE-THRESHOLD BY THE PSEUDOSCOPIC METHOD.

Recently my attention was called to an article by Professor Stratton which appeared in your issue of November, 1898, and which I found contained a serious error. Professor Stratton came to the remarkable conclusion that binocular relief was still perceptible even when the images which produced it had a separateness of less than 24", or in other words that our sense of binocular perspective was more delicate than our visual acuity. It seems to me that if this is true it must overthrow our ideas as to the rods and cones being the ultimate visual units. A glance, however, at his method of reaching this result reveals the source of his error. He has compared pseudoscopic with stereoscopic vision without allowing for the fact that this is practically equivalent to doubling his interocular distance. His results therefore should be multiplied by two, which gives 48", and this, according to Professor Stratton's own statement, closely agrees with the results obtained by other observers.

F. H. VERHOEFF.

The objection which Dr. Verhoeff raises to my results with the pseudoscope, in so far as he bases it on their *a priori* improbability, will perhaps be met to some extent by my paper in the September number of this REVIEW, which has appeared since his communication was written. By an independent method it is there shown that spatial discriminations by the eye may go as low as 7" of arc, and thus be considerably less than the suspicious 24". And I have tried to show that even this smaller value may still be harmonized with the assumption that the rods and cones are the ultimate elements of vision.

As to the computations in the pseudoscope paper, I feel that Dr. Verhoeff's criticism is due to a misunderstanding of the essential features of the experiment, for which some obscurity in my report is doubtless to blame. His objection would seem to me valid if the depth-effect noticeable at 580 meters had been *produced* by the rapid alternation of pseudoscopic and stereoscopic vision. For at the very moment of transition there might well be a summation of the eye-movements peculiar to each of these kinds of sight, and consequently a movement double that which was present in the normal stereoscopic view. But in the original experiments I myself never got the impression that the alternation itself called the observed depth-effect into existence. The pseudoscope contributed merely a 'flat' standard, by comparison with which we might more readily convince ourselves that what we saw in the normal scene was truly stereoscopic relief, and not something due merely to the vivid coloring or the light and shade.

In view of Dr. Verhoeff's objection, however, I have repeated the experiment and feel assured that this original impression was correct. That the plastic effect in the distance is not a product of the transition is clear from the fact that it does not fade away when the transition itself is past and gone, but persists indefinitely in the normal binocular view. Even a day after the pseudoscope has been laid aside the relief is unmistakably present at 580 meters and more. And what is perhaps still more convincing, this same stereoscopic appearance, one can recall, is noticeable even before the pseudoscope is put to the eyes, although the observer then lacks the subjective assurance that his observation is correct, which comes the moment later when he sees the same scene deprived of this stereoscopic character.

The depth-effect at 580 meters is thus evidently produced by conditions lying wholly within our normal binocular vision, and the rôle of the pseudoscope in my experiment was not to add to the angular disparity of the two images and so aid in producing this effect, but merely to facilitate the recognition of the plastic character present

in the normal view all the while. The computation of the angular disparity which gives this stereoscopic relief should consequently be based on the actual interocular distance, and not on double this distance, as Dr. Verhoeff maintains.

GEORGE M. STRATTON.

UNIVERSITY OF CALIFORNIA.

A MAGNET REGISTRATION KEY.

The writer has for several years noticed a special difficulty in the use of break-circuit keys in reaction-time experiments, namely, that there was a tendency, especially among those who were just learning or who had not had recent training, to press the button so hard while they were waiting for the signals as to open the circuit, and so frustrate experimentation. This fault recurs also from time to time with subjects in fairly good training, especially if they are using the shorter modes of reaction. In some cases it has even seemed necessary to have them keep the hand just above the button and not in contact with it till the habit of light pressure was established.

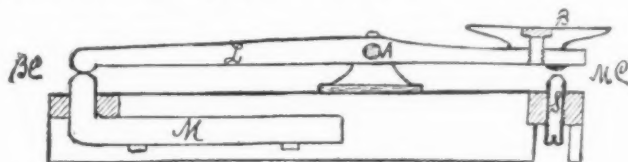
The reactors themselves will not readily believe that they are making the trouble by pressing too hard on the key and show surprise when it is demonstrated that they do. A break in some other part of the circuit or a premature reaction is frequently erroneously suspected. The trouble was particularly pronounced—in this case with trained subjects—in some experiments in which a break key was used as a shunt, perhaps because, even if the circuit was not actually broken, the pressure on the key increased the resistance in it so far as to send the current through the chronoscope.

The explanation of the fact is probably this: The lever of the usual form of key is held against the fixed contact piece by a spring. When the button is pressed down, the spring offers a continuously increasing resistance to the movement, and the contacts may separate slightly without giving any distinct feeling of the transition from contact to separation, by which the subject may be guided. There is, perhaps, also a tendency especially in the short or motor forms of reaction to secure a definite sensation from the pressure on the button and to exert such a pressure on it that the smallest possible addition will break the circuit and so make the reaction shorter. In the absence of a distinct warning signal that the contacts are separating, the pressure on the button can easily become too great and may be much greater than the subject himself is aware. It is of course not impos-

sible that the pressure may in some cases be of a still more involuntary and automatic character.

This trouble has not been observed in cases in which the registration key was used to close the circuit except when the distance between the make-circuit contacts was made very small, so that only a slight movement could be made and the shock of contact was inappreciable under the conditions of the experiment. If, on the other hand, the distance between the contacts is large a considerable error is introduced, as is doubtless also done by varying within wide limits the amount of pressure of the spring or hand on the lever.

Accordingly, it occurred to the writer that if a key were constructed that would hold the contacts together sharply and give a marked warning when they begin to separate, the difficulties and inaccuracies above mentioned might be much reduced if not wholly removed. As is well known, the contact of a piece of iron or steel with a magnet gives exactly the conditions desired, and the instrument figured below in a longitudinal section through the center, in which the contact surfaces are held together by magnetism instead of by a spring, serves the purpose in view in a thoroughly satisfactory manner.



Magnet Registration Key.

The lever *L*, which is about seven and one-half inches long, is made of soft iron and rests at *BC* on the small permanent magnet *M*. It is so cut and balanced that the end *AL* is a little heavier than *AB*, so that however great the movement of the lever the forward end of it will have a slight tendency to fall toward the magnet, by which it will be caught and held fast on contact. To avoid friction or binding, the axis *A* works loosely in the bearings, and its contact with the bearings is not depended upon for the transmission of the current, but this is secured by a couple of spirals of wire from the axis to the metal supports. The button *B* is made large so that the ends of three or four fingers may be placed upon it. The contact for breaking circuit is at *BC*. In the instrument made by the writer, the surfaces of contact were left plain iron or steel but they might be improved by covering with thin sheets of platinum, if a stronger magnet were used, and yet a sufficiently distinct warning be given on the separation of the contacts.

The make-circuit contact is at *MC*. The top of the screw *L* may be brought to within quite a small fraction of a millimeter of the contact projection of the lever and yet the shock of separation from the magnet be distinctly felt. A rather close approximation to the accuracy of a break-circuit contact may thus be obtained.

The strength of the permanent magnet is a matter of importance. The one used in this case is made from a one-fourth-inch steel rod, five inches long, and seems fairly suitable.

When the key is used for break-circuit purposes, a soft-rubber cap may be placed over the top of the screw *L*, and a movement of two or three millimeters be allowed so as to make the sensations very distinct without injuring the make-contact surfaces.

The sensation of breaking contact with the magnet is so definite that the beginner quickly learns what force to employ, and any mistake made by such a person or a more experienced reactor is known at once. By breaking contact occasionally between experiments to get an idea of the maximum pressure permissible and desirable a rather uniform standard may be maintained throughout.

The making of the sensations and innervations connected with the registration of reactions in the way described more definite may prove to be a matter of some importance as well as convenience in psychological experimentation. A special use may also be found for the key in the observation of important occasional events, astronomical or other, in which great accuracy is desired, but in which lack of training or excitement might interfere with the result.

JOHN A. BERGSTRÖM.

UNIVERSITY OF INDIANA.

PSYCHOLOGICAL LITERATURE.

The Spiritual Life, Studies in the Science of Religion. By GEORGE A. COE, Professor of Philosophy in Northwestern University. New York, Eaton & Mains; Cincinnati, Curts & Jennings. 1900. Pp. 279.

This volume is an important representative of the recent movement to carry the methods of science into the study of the facts of the religious life. It is an empirical study, in which a fund of distinctly psychological data furnishes the groundwork for bringing into relation and into perspective many apparently isolated phenomena of religion. The analysis and synthesis of spiritual facts among themselves and in their connections with facts of non-religious character are done with discrimination and skill.

Perhaps the chief interest of the volume and that which especially makes it a valuable contribution to the psychology of religion is the study of temperament, both as a factor in the variety of religious experiences and in the determination of the peculiar types of religious expression. Personal records were obtained from 77 persons whose experiences had either contained striking religious transformations or an unfulfilled expectation in regard to them. These persons were questioned also as to likes and dislikes, laughter and weeping, anger and its effects, habits of introspection, moods, promptness in decisions, ideals, the effects of excitement, habits with respect to physical activity, etc. In addition to reliance upon this self-analysis, Professor Coe made an exhaustive study in various ways of the individual cases. Objective observation of the written records and also of the habits and peculiarities of the persons studied was brought to bear. Of especial value was a series of tests from the use of hypnotism and suggestion.

The cases formed two main groups: persons who expected a religious transformation and experienced it, and those who expected but did not experience it. In regard to each of these groups Professor Coe raised the question, in the first place, as to whether intellect, sensibility or will was predominant. He ascertained that "where expect-

tation is satisfied, there sensibility is distinctly predominant; but where expectation is disappointed, there intellect is just as distinctly predominant." "A second interesting result is that those whose expectation is satisfied belong almost exclusively to the slow-intense and prompt-weak varieties, the temperaments approaching most nearly those traditionally known as the melancholic and sanguine. On the other hand, those whose expectation is disappointed belong more largely to the prompt-intense variety, or the choleric temperament."

In studying the relation of these experiences to mental and motor automatisms, it was found that 65 per cent. of those who had had a striking transformation had also exhibited automatic phenomena, while only 8 per cent. of those who expected a transformation in vain had had either an hallucination or a motor automatism. In other words, "the mechanism of striking religious transformations is the same as the mechanism of our automatic mental processes."

The most significant and the most unequivocal results came from the study of the relative susceptibility of the two groups of persons to the influence of hypnotic suggestion, and their action under it. The question was as to the parts played by external suggestion and by auto-suggestion upon these two classes of subjects. Among those whose expectation of a transformation was gratified, 13 out of 14 were 'passive,' *i. e.*, they were suggestible, but exhibited little spontaneity under the operator's influence. Of 12 persons whose expectation was disappointed, all but one may be said to have belonged to the 'spontaneous' type. Professor Coe points out in a forcible way the bearing of these facts upon the phenomena of revival meetings, trances, visions, miracles, etc.

The chapter on 'Divine Healing' is farther removed from empirical data, but is carefully analytical and convincing. The last chapter, 'A Study of Spirituality,' follows up the thread of the influence of temperament in religion. "The conclusion upon which all these diverse lines of investigation converge is that organized Christianity in general, Protestant as well as Catholic, places insufficient value upon the more masculine, active or practical qualities of goodness; or, to speak in directly psychological terms, that the forms of religious life natural to the choleric temperament are habitually discounted in favor of those natural to the sanguine and melancholic temperaments, particularly the latter."

The style of the volume is clear and attractive. The book is a valuable contribution to both science and religion.

EDWIN D. STARBUCK.

The Psychology of Conjuring Deceptions. By NORMAN TRIPLETT.
Am. Jour. of Psych., XI., 4, pp. 439-510. July, 1900.

Mr. Triplett presents a very comprehensive survey of the various aspects of conjuring which contribute data of psychological interest; and these are exceptionally diverse and instructive. Viewed genetically, the instinct to deceive, and later the pleasure in the exercise of deception, may be considered as expressions of helpful developmental traits; guile and cunning, craft and strategy contribute to the fitness that survives. Even mimicry involves the deception of the animal that preys; although it is obviously not a psychological reaction of the preyed upon. From this to death-feigning, to purposive strategy and the war of wits, to the recorded cases of shirking among domesticated animals, to the unconventional lies of children and the conventional ones of society, there is an irregular but mutually illuminating series of reactions in which deception plays an important though variable part. Conjuring as an art likewise opens out an historical background, in which appear the magic rites of savages, the ceremonies of primitive religions, the beginnings of physical science, the increasing control over the resources and energies of nature. The dual aspect of deception, the fact that there must be deceiver and deceived, demands attention to the processes by which the latter is misled equally with the devices by which the conjurer mystifies. Indeed the more elaborate forms of conjuring become far more psychological than physical, depend far more for their success upon the induction and suggestion of the proper mental attitude and inferences on the part of the spectators, than upon mere dexterity or mechanical ingenuity; and herein lies the great value of this material to the student of the psychology of deception.

Mr. Triplett's study places this material at the disposal of the psychologist in far more convenient and well-arranged form than has hitherto been done. His elaborate list of classes of conjuring tricks, while inevitably involving cross-classifications and very disparate rubrics, furnishes a satisfactory guide to the more essential principles which these tricks involve. A dominant principle, most frequently illustrated, is the kinship of conjuring to suggestion; for it is the suggestion of things not done quite as much as the concealment of those that are done that determines the success of modern conjuring. Of the conjurer as of the diplomat it is true, as Mr. Triplett cites, that "he says what he does not do, he does not do what he says, and what he actually does he takes particular care not to say anything about." Extreme suggestibility leads the way to hallucination. When a ball

was tossed in the air twice, but on the third time was allowed to drop behind a desk (the hands completing the motion as though the ball had really been tossed), 40 per cent. of the school boys and 60 per cent. of the girls who witnessed the test saw the ball go up and disappear at the third (suggested) throw. From this simple suggestion to the more complex ones that dominate stage illusions, or that invest the spiritualist's séances with mysteries of his own creation, the transition is psychologically close. So in the end the study of deception involves the study of suggestibility, of credulity, of mental contagion.

Interesting as may be the general trend of the argument thus outlined, the interest is markedly increased when applied to the analysis of concrete examples. The unexpected and ingenious application and combination of principles arouses admiration when the trick is revealed, and profound mystification when it is performed. For such detailed illustrations of the psychology of conjuring, Mr. Triplett's able study will serve as a useful compilation and exposition.

J. J.

Beiträge zur Entstehungsgeschichte der neueren Ästhetik. By WILHELM KUNTZ. A doctor's dissertation at the University of Wurzburg.

Dr. Kuntz divides the development of modern æsthetics into three periods. The first of these is the period of preparation in the psychology of the Middle Ages as well as in the conceptions of nature and art characteristic of that time. Elements akin to the concept of sublimity are to be found in stoicism and still more in early Christianity. The concepts of the fantastic and the humorous were not found among the ancients.

The allegorical character of mediæval art was due to emphasis on the ethical and hostility to pagan art. In the psychology of this period Dr. Kuntz believes that he finds the first traces of a theoretical foundation of the symbolic. The passages referred to in the works of Augustine and Thomas Aquinas give slight ground for his conclusion. But St. Augustine does contribute to the development of the concept of unity in the manifold in his recognition of the æsthetic value of the ugly.

The second period is that of the Renaissance. Dr. Kuntz traces the rise of classicism in art and criticism as typified in the dramatic canon of the unities.

Part III. deals with the rebellion against classicism and the beginnings of the return to the natural. It is characterized by the grow-

ing importance of the concept of fancy and the union of the ethical and the æsthetic, and by opposition to the dramatic canon.

Dr. Kuntz would find a dialectical development from the æsthetic point of view among the ancients through the ethical point of view in the Middle Ages to a union of the two in the present. One might argue that a selection of authorities differing from that of Dr. Kuntz would give a very different conclusion.

The dissertation as a whole is a very good summary of the earlier development of modern æsthetics.

A. LEROY JONES.

THE ANALYSIS OF SENSATIONS.

Berträge zur Analyse der Gesichtswahrnehmungen. Von F. S. SCHUMANN. Zeitsch. f. Psych. u. Phys. d. Sinnesorg., Bd. 23, Heft 1 u. 2, pp. 1-32.

This is the first of a series of articles that Professor Schumann proposes to devote to the results of an introspective analysis of the more complex mental processes. In the introduction the author justifies the undertaking, that scarcely needs justification, with the statement that the exploration of the field of sensation which must form the foundation of all experimental psychology, even though many of the results fail to find a permanent place in the science, has now been carried very far, and it is time to begin to extend the investigations to the processes that lie behind the sense organ and which must eventually form a large part of the superstructure of the discipline. Many suggestive remarks are made on the place of introspection in psychology and the safeguards that must be thrown about its use if errors are to be avoided.

The author chooses for his first contribution a study of the effects of the attention upon simple patterns and through that an investigation of the nature of visual form. The investigation begins by noting how from simple patterns it is possible to pick out a series of different figures by emphasizing first one feature then another by the attention. For instance, a number of straight lines drawn parallel can be grouped in twos and seen as a series of laths with spaces between as easily as they can be seen as single lines. Lines of print are grouped in threes when counted, and each three becomes a unit, perceptual as well as conceptual.

Similarly a mass of squares can be seen in many different ways by the arbitrary action of the attention, as groups of twos, fours or sixteens. The black squares may be made dominant or the white lines

that separate them, and the white lines can be made to take on any pattern at will; but whatever pattern they assume, that for the moment holds the attention and constitutes the object that is seen.

A similar line of inquiry is carried out with reference to the perception of simple forms. So, for instance, it is shown that a square differs from an oblong because in the square each line claims consciousness in approximately equal degree, while in the rectangle the two longer sides claim the attention to the exclusion of the shorter.

Again, a square with a diagonal vertical makes a different impression from a square on its base because in the one case the vertical diagonal divides the area into two parts and two adjoining sides are united in perception, while in the other case the opposite parallel sides claim the attention together. That this is the explanation can be seen more clearly from the fact that attending to one corner of the square upon the base makes the diagonal drawn from that point dominant in consciousness, unites adjacent sides in a single whole for consciousness and makes the figure identical for perception to the square with its diagonal vertical. Numerous other simple geometrical figures were shown to owe their peculiar form to the way in which their separate elements are conjoined in attention. These observations seem amply sufficient to justify Schumann's theoretical conclusion that visual geometrical form is for perception not as Meinong and Ehrenfels have insisted a new *qualé* added to the spatial ultimates, position, direction and extent, but a new way of perceiving or arranging these spatial elements, the recognition of a new unity among them. The form depends upon the method in which the elements are grouped by the attention. What is meant by this term unity is made clearer by the statement of its three concrete marks.

1. Within a group there is immediate recognition of unities of varying degrees, just as there is immediate recognition of the varying degrees of unity that result from tone fusion. A series of ten circles is more like a homogeneous straight line if seen as a single whole than if perceived as five groups of twos.

2. The effect of a group upon the attention is more unified if the group is seen as a whole at a glance, than if each of the elements is considered for itself, and first one and then the other catches the attention.

3. Each group has a different effect in recalling ideas from every other group.

While these marks serve to point out rather than to prove the nature of the unity, they are sufficient for the attentive observer, and furnish

as clear a demonstration as can be expected with reference to an ultimate element of experience.

W. B. PILLSBURY.

AFTER-IMAGES.

Beobachtungen über den Verlauf centraler und extramacularer negativen Nachbilder. ANTON WALTHER. Pflüger's Archiv, 77, 53, 1900.

By means of an arrangement of holes in screens, round spots of any color or degree of brightness (and surrounded by any sort of a background) could be presented to the eye for central and for eccentric vision at the same time, and thus the course of the after-image under these two conditions could be conveniently compared. Many interesting points of difference were found to obtain. It seems from these observations, as from so many others, that the process for color in the visual organ is something distinct from that for the achromatic sensation; after the colored after-image has faded out there is a colorless residue which persists for some time longer. The excitation, whatever it may be, which is the basis of the after-image is distinctly different for red and green on the one hand, and for blue and yellow on the other; in the case of red and green, the double-process [if we may use this term for the persistent (positive) image and its negative successor taken together] runs its course in much the shorter time; thus if the color presented be a yellow-green, the purple, which is the after-color to the green in the mixture, will soon lose its red and the after-image will seem much too blue. So if the original color is violet or blue-green (peacock), its after-image will be over-yellow. (This doubtless accounts for the fact, frequently stated, that an after-image is not always seen to be strictly complementary in color to its primary.)

If one gazes steadily at a colored surface it becomes quickly whitish on account of the retinal impression being overlaid by the already developing after-image, and if the illumination be diminished the opposite color will even appear in place of the actual one, though the eyes are open; here also, when the hues are not the primary ones, an excessive amount of blue or of yellow will appear in the final result. In all these instances the greater duration of the blue and the yellow is still more marked in the extra-macular region than in the center. (This may be expressed by saying that there is poor red-green vision in the periphery, and that in the after-image it gets poorer still.)

If the original impression be central, the after-image (it is the negative after-image that is in question throughout this paper) does not set in at once—it undergoes a gradual development and then a gradual fading out. But in the case of the excentric image this preliminary pause and gradual development could not be noticed—the image appears at once, and with full saturation; at this time, therefore, the excentric image is both brighter and more saturated than the central one, but on the other hand it fades out much sooner. It is known that the after-image goes through periods of brightening and darkening, and even of vanishing and returning; this feature also is more marked in the periphery.¹ Walther confirms the observation of Hering that several images can be started up one after another, and can then be seen to run their different courses at the same time. This completely eliminates, of course, all the effects of a mistaken judgment. This investigation adds another confirmation to the theory regarding the disjunction of function of the rods and cones: it has already been said that there is a colorless constituent of the total sensation which runs a different course from that which furnishes the color; but if the image of the spot is strictly limited to the rodless region of the retina (2° in diameter—an area of 2 mm. across at a distance of 25–30 cm. from the eye), as may be done by the method of holding the object, centrally fixated, within a ring of other spots, this difference completely disappears (*i. e.*, the dissociated gray constituent of the image no longer exists). It will be seen that this investigation furnishes a very interesting addition to our knowledge of the after-image.

C. LADD FRANKLIN.

Die Erscheinungen bei kurzer Reizung des Sehorgans. By HERBERT MUNK. *Zeitsch. f. Psych. u. Phys. d. Sinnesorgane*, XXIII., 60–100.

The article is a study of after-images, especially emphasizing the important and hitherto quite neglected influence of the background. The separation of the sensation from its after-image by the so-called latent period, as well as the break between the phases of the positive after-image, is found to become less marked as the contrast between the stimulus and its ground decreases until they entirely disappear; and the after-image appears as a unitary process, continuous with the sensation. Not only the breaks in the process, but the modifications of the positive after-image as it gradually disappears are shown to be the effect of the contrast between the main stimulus and its ground, which

¹ But see results of Munk, to be noticed later on this point.

becomes more pronounced during the development of the after-image than at its beginning.

The theoretical discussion is a cautious attempt to explain the development of the contrast in the after-image through the diffusion of light, on the schema of G. E. Müller's general theory.

The author's experiments with colored stimuli, while making no pretensions to exhaustiveness, are entirely congruent with the foregoing. The complementary phase of the positive after-image is found to disappear entirely when the colored stimulus has for its ground a neutral gray of like brilliancy.

RAYMOND DODGE.

WESLEYAN UNIVERSITY.

THE EAR, HEARING AND ORIENTATION.

Ohr labyrinth, Raumsinn und Orientierung. E. von CYON.
Pflüger's Archives der Physiologie, Vol. LXXIX., Nos. 5 and 6,
January 31, 1900, pp. 211-302.

This elaborate and interesting research on the bodily organs correlate with the sense of space and the faculty of orientation is rich both as a report of experiments and as a summary of the opinions of others who have investigated these rather important subjects. The article is divided into seven divisions with the following titles: (1) introduction; (2) observations and research on Japanese dancing-mice; (3) rotation experiments on children, apes and tortoises; (4) separate observations and research on the orientation of carrier pigeons; (5) geotropism, heretofore the 'static sense'; (6) conclusions as to the sense of space, and the normal stimulus of the labyrinth of the ear; and (7) historical account of the sixth sense.

The experiments of Professor von Cyon were made on a peculiar variety of mice, on lampreys, children, apes, tortoises, and on carrier pigeons, but chiefly on the mice. The interest of these animals lies in the fact that they have practically only one, namely the 'uppermost' of the three semicircular canals which most of the higher animals possess [see a description by B. Rawitz in *Archiv für Physiologie*, 1899, Nos. 3 and 4, pp. 236-243]. When they walk their direction is oblique, and when they run it is always in a circle. By 'labyrinth,' the author means the semicircular canals with their ampullæ and the sacculus, and does not include the cochlea, etc., as is often included in the labyrinth by anatomists. The otocysts of molluscs, crustaceans and other low forms are considered the homologues of these

canals. Space cannot well be afforded for description of the numerous and interesting experiments. Those made on the Japanese dancing-mice are in seven divisions, discussing respectively their peculiar movements, the cause of these movements, the hearing-organ of the mice, the direction of the mice's movements, a 'blindfolding-research' on the mice, their power of equilibrium and of coördination, and a rotation-research made upon the animals.

According to this investigator the labyrinth in general is the organ by whose functioning the animal orients itself in space, especially as regards the maintenance of equilibrium. The labyrinth from which this power of orientation arises sends both inhibitions and stimulations to the cerebral centers which have to do with regulating the innervation of the eyes, the head and the back. These currents, by coming from both halves of the body at the same time, tend largely to keep them in equilibrium. This maintaining of the equilibrium, whether when standing normally or when lying or in any other position than these, seems to the author 'only a special case of orientation in space.' All of these depend on the intervention of the same nerve-centers; all occur in space, and all are connected with changes in the relations which the whole body or any of its parts bear to the three dimensions of this space. The difference which has commonly been made between static and dynamic equilibrium or orientation and the need of searching for separate organs for these are therefore wholly needless. The maintenance of equilibrium is in part due to the noises which are continually falling on the ear, that of the heart-beat for example being constant should those of extra-bodily origin fail. It is not difficult for von Cyon to carry this matter still further and to suppose that the noises act as continual stimuli possibly on other nerves as well as of the labyrinth, namely, on those connected with the short hairs of the macula acustica in the sacculi. The acoustic hairs of the ampullæ are stimulated especially through tone-generating vibrations of the air and help in this way in the estimation of the direction of sounds and so in the production of spatial sensations. The stimuli which bring about orientation in space act only temporarily, while those which allow of equilibrium are continuous.

Orientation at a distance, a faculty common to very many sorts of animals and notably to the carrier-pigeon, seems to von Cyon to depend not on reflex and instinctive processes, but on those that are 'premeditated and conscious.' In this faculty the labyrinth is only accessory, sight and 'a special direction-sense located in the mucous membrane of the nose and perhaps also in that of the frontal sinuses'

(which are connected with the nose by the infundibulum, in the case of man) being also concerned; he considers that the sense of smell or some related influence may help in the faculty as well. The direction, speed, temperature and other qualities of the wind seem to play a large part in this faculty in the case of a bird flying from one town to another. A useful list of books which treat of these subjects up to the present time is to be found at the end of the article.

GEORGE V. N. DEARBORN.

TUFTS COLLEGE MEDICAL SCHOOL.

EXPERIMENTAL.

Ueber die Beeinflussung der Auffassungsfähigkeit durch einige Arzneimittel. NARZISS ACH. Psychologische Arbeiten (Kraepelin). Bd. III., 2 Heft, 1900. S. 203-288.

The writer subjected various medicaments to systematic test with a view to a quantitative estimation of their influence upon the process of perception and the capacity for retention. The drugs used were alcohol, bromine, paraldehyd and coffee. The work was done in 1897, part at Heidelberg, the rest at Strassburg. The method followed was that employed by Cron and Kraepelin in analogous experiments. Three visual series were read through at each exercise, each series consisting of nonsense syllables, monosyllabic words and disyllables. In each experiment the percentage of errors of various kinds occurring in the reading of the material, presented by a revolving drum, was compared with percentages occurring when the reacting subject was under the influence of the drug. In the case of coffee the dose was administered half an hour before the beginning of the experiment, normal and drugged conditions being tested on separate days. In experiments with the remaining drugs both phases were explored at the same exercise, the drug being taken at the close of the normal period and a pause of ten minutes occurring before proceeding with the rest of the drug state. The use of alcohol was avoided throughout the investigation, and that of coffee within six hours of the test.

As concerns their influence upon psychic processes the drugs fall into three classes. The first comprises alcohol and paraldehyd, the effect of which is injurious; the second, bromine, affects the activities appreciably in neither direction; while coffee, taken in moderate quantities, works an undeniable improvement in the perceptive faculty.

The influence of the first group is shown in a lowering of the capacity for work. This appears in two forms: first, an increase above the normal in the number of elements which are overlooked

and omitted; and second, an increase in the number of misreadings. Deterioration in the perceptive activity will be most unequivocally exhibited in those processes in which the factor of memory and association is reduced to its lowest value, namely, in the reading of nonsense syllables; and it is here that the increase in errors reported in the experiments was most marked. This effect of alcohol and paraldehyd had already been inferred by Kraepelin from his experiments in discrimination and reaction times. In the case of both of these drugs the group of letters which constitutes a word is no longer apprehended as a totality, but tends to break up into more primitive rhythmical combinations in which the first, third and fifth letters are favored, while the second, fourth and sixth are overlooked.

These drugs bring about both a retardation of the perception process and a narrowing of the span of apprehension, which produces a feeling of increased rapidity in the rate at which the letters pass before the opening. The successive phases of action in the two drugs present important differences. The effect of paraldehyd is more intense and more transient than that of alcohol. The impairment of perception due to the latter appears about ten minutes after taking the drug, and rises slowly to a maximum attained at the end of half an hour; while the action of paraldehyd is shown in seven minutes, and its acute stage is reached in fifteen to twenty minutes.

With increased practice the alcoholic interference passes rapidly away. This is especially noticeable in the reading of words, in which the association factor may play a great rôle. In general the memory suffered no apparent injury through the use of alcohol or paraldehyd, and the results may be regarded as harmonizing with those of Kraepelin, who found the former drug to bring about initially an increased facility in learning numbers. The prevalence of a reappearance of the same verbal combinations in mistakes made under the influence of these drugs is worth noting.

The experiments with bromine presented practically negative results. The action of coffee as evidenced by these tests is favorable to the activity of visual perception. Its effect appears especially in a reduction of the number of omissions, but is shown also in a lowering of the number of misreadings and misreproductions. With the quickening of apprehension goes also an increased accuracy of retention.

The temporal phases of coffee are not so marked as those of alcohol and paraldehyd. Its action is less intense than theirs, and more sluggish. Its effect becomes noticeable fifteen or twenty minutes after taking, and even after an hour and a half is still observ-

able. Here, again, the results of previous experiments are corroborated: Diehl and Vintschgau found a striking and long-enduring reduction of reaction times under the influence of coffee, and Kraepelin reports a facilitation of the apprehension of external impressions through its use, as well as the process of habitual association.

The results of these detailed experiments support the popular conception of the effects of this group of drugs and its medical application. Coffee causes a moderate rise in general mental activity, which but slowly passes away and is manifested in an increased capacity for work and pleasure in it, while at the same time it postpones the advent of fatigue and sleep. The value of bromine lies in a quieting of excitement characteristic of certain diseases, and its action seems a variable independent of the general mental activity. Paraldehyde produces a swift effect which shortly passes away, and is, therefore, adapted to the purpose of inducing sleep. The effects of alcohol manifested here are identical with those more striking forms of perversion and injury which accompany the drinking bout. The misapprehension of speech is here, the incapacity for analysis and discrimination, the premature fatigue, the uncontrollable desire for sleep. Moreover, it is the same functions which are most intimately interfered with here as in social drinking. The activity of selective consciousness suffers most, habit and the mechanical processes of the subject are less affected. In the pathology of chronic alcoholism this slowness and uncertainty of apprehension appear in extreme degree, yet it is but the extension and fixation of results which these finely quantitative experiments show to ensue upon even the very moderate use of alcohol.

Rhythmus und Arbeit. MARGARET KEIVER SMITH. Philosophische Studien, Bd. XVI., 1 Heft, 1900. S. 71-133.

Miss Smith's article is a report of work carried on during the year 1898 in the laboratory of Professor Meumann in Zürich. The account is incomplete, giving only the results of the experiments serially, and leaving for a later article the summary of the work and its bearing upon pedagogical and other problems. The object of the investigation is to determine by a series of simple mental tasks under what condition the carrying on of work rhythmically improves its quality or increases its quantity over that of similar work done arrhythmically.

It is to be noted, however, that the experiments did not strictly set in contrast phases of activity which were respectively rhythmical and non-rhythmical, such as would be attained by carrying on both pieces

of work in obedience to objectively given periods, which were regular in the one case and irregular in the other; but compared work done under direction of a rhythmical set of signals with that performed freely in natural periods without conduct of any kind. The existence of rhythmical activity in the latter case is of course a possible hypothesis, and much evidence goes to confirm the belief that natural rhythms are widely characteristic of physical and mental work. As a consequence of the comparison of the results here reported with work done under control of arrhythmical objective periods we should look for an increase of the margin of effectiveness which the writer found, within the scope of her experiments, to exist in favor of work rhythmically conducted.

The investigation comprised five problems, beginning with simple muscle work and proceeding through a series in which the mental factor was of progressively increasing importance. The first exercise consisted of ergographically recorded movements of the middle finger, both in extension and reflexion, continued until the appearance of complete fatigue. In the second group simple curves, letters and syllables were written continuously for a fixed period. The third set compared the accuracy in discrimination of slightly differing weights when made without and with observance of a rhythmically given signal. In the fourth the influence of rhythm upon the process of committing to memory series of letters and nonsense syllables was studied. The fifth and last group comprised a series of experiments in which the motor factor was reduced to the smallest attainable value, and the effect of rhythm upon 'pure visual' acquirement was investigated by a method devised to meet the conditions of the problem. Neither of the last two groups is reported upon in this paper.

The results obtained in the earlier series are in abstract as follows: In regard to the first group nothing satisfactory is to be reported. The work done without objective control so rapidly fell into a completely rhythmical form that a comparison of rhythmic with non-rhythmic activity was out of the question. The quality of the work becomes a little more regular under guidance of the metronome, and in some cases more work is done with than without it.

Half a dozen variations of the second exercise—writing letters and words—were gone through with, which showed a fairly constant, though slight, balance in favor of the metronome accompaniment. In connection with these writing exercises it is to be remembered that each person has his own characteristic tempo, and any computation of the influence of rhythm must take into account

the possible interference of the objective rhythm with this natural period. The conflict of these tendencies appears in a deterioration in the quality of the work done. When the experimenter loses his natural rhythm and is unable to fall in with the new period, the favorable influence of rhythm is lost, and its effect may be distinctly fatiguing. Indeed, on the basis of these experiments little can be said in general concerning the influence of rhythm upon the quality of the work done. Upon the quantity of work done rhythmical conduct shows a consistently favorable effect.

One set of experiments in which the unit written consisted of the word 'command' resulted in a curious failure. Both of the two subjects accomplished more work without the guidance of the metronome than with it. The result was apparently not due to any unnaturalness in the period of the metronome, since many varying rates were tried. The experiments were soon given up. The influences of rhythm have thus evidently wide individual variations, and it would be interesting to know more in detail concerning the periods in which these subjects naturally wrote. It is conceivable that here we are met with cases of complex natural rhythm of alternate phases instead of the absence of periodicity. To such a form of reaction no metronomic period, however varied, would conform.

The introduction of variations in the rate of the metronome from forty beats per second up to one hundred and sixty showed a fairly well marked maximum of effectiveness or fitness occurring within a range extending from eighty beats per minute to one hundred, in accompaniment to which the greatest amount of work was done.

The introduction of rhythmical guidance into the process of comparing lifted weights produced effects analogous to the preceding. It is marked by an increase in the percentage of correct judgments over that returned without such accompaniment. The gain is small, in one instance having a value of $99\frac{3}{4} : 97\frac{1}{4}$, in another that of $89.5 : 88.7$, in a third showing no influence favorable or unfavorable.

Throughout these experiments the influence of the metronome apparently was shown in an attitude of increased expectancy and a heightening of the attention process. We should, therefore, possibly refer at least part of this small advantage of the rhythmically conducted exercise over the free activity, directly to a quickening of the attention process, which is independent of rhythmical functioning and may be aroused by any form of mentally exciting stimulus. Even when found unpleasant this increased tension of the perceptive or motor process was noticeable and promoted the tendency to increased production.

In a final series of experiments in which one subject took part the influence of movements free in extent as well as time was contrasted with that of reactions controlled both in period and extent. In his free movements the subject raised the weights a full meter high, and the exhaustive character of the exercise was shown in the enduring fatigue which ensued. When the upward lifting of the weights was limited to a movement ten centimeters in extent—as in all preceding sets of experiment—the subject became distrustful of his judgment and found the restraint unpleasant. Though the results were ambiguous the test fulfilled its purpose in that the free movements of the subject showed a well-marked tendency to maintain a rhythmical period in the lifting of the weights.

ROBERT MACDOUGALL.

HARVARD UNIVERSITY.

Psychological Studies. By HARLOW GALE. Number one. July, 1900. Pp. 175. Published by the author, Minneapolis, Minn. For sale by H. W. Wilson, University Book Store, Minneapolis, and Berhn. Liebisch, Leipzig.

The first study on 'Our Nervous System and its Use' is a condensation into 38 pages of the author's four or more lectures and demonstration with original lantern slides, in which the object is to show the latest and most reliable pictures of the brain elements, their connections and their probable functions. The reason for this effort by a psychologist is because the modern anatomy and physiology of the nervous system are so scattered and difficult of collection, and because of the immense importance of some first-hand knowledge of the nerve elements for widening the too-often narrow bounds of the psychological student. For demonstration material in this article there are given 21 original photo-micrographs in half-tone and 29 reproductions of schematic figures and plates, with photographs by Flechsig, Cajal, and His. The summary of the article is as follows:

1. The means of taking in the stimuli surrounding our bodies is the sensory system of bipolar cells situated either within the end organ of sense or in groups (ganglia) at various distances between the sense organ and the cerebro-spinal system. The uniform plan of the sensory cells is one fiber growing out to the surface of the body to receive through the sense organ the stimulus, and the other fiber growing into the cord or brain to conduct the stimulus therein.

2. The cerebro-spinal nervous matter consists of: (a) intaking fibers of the outlying sensory cells, (b) motor cells with their outgoing

fibers and (c) a mass of cells serving to connect together the different sensory stimuli and to connect these associated stimuli with the outgoing motor cells, these different grades of interpolated cells having all the general function of association.

3. The embryological evidence confirms this simplified plan of three functionally different cells, viz., (*a*) sensory cells outside the cord and brain tube, (*b*) motor cells going out of the tube and (*c*) association cells within the tube.

4. The specialized function of the small brain is to make the important associations for our complicated motor reactions and expressions.

5. The specialized function of the large brain is to make associations of the stimuli from the surrounding world so that thinking and feeling result, whereby reactions are made with more prevision for our welfare.

6. The wonderful advance of the last dozen years in our knowledge of our marvellous brain cells has been in disclosing an inconceivable possibility for association, which is the great psychological basis of our mental life. So that with an ever-increasing wonder on seeing real pictures and details of this most intimate and beautiful mechanism, its earnest student can share some of the satisfying and yet stimulating strength of Flechsig when he concludes the preface of his epoch-making work—"More and more have I the conviction that the brain covers fully and completely the facts of the mental life as their organ, and that we are in a situation to develop the conditions of this dependence with the same exactness as in the other phenomena of nature accessible to our knowledge."

Connected with this article, though at the close of the volume, is a catalogue of 126 reproduced schematic cuts and plates, and of original photo-micrographs of nerve cells and tracts; prepared as lantern slides for lecture use in demonstrating the histology, physiology and psychology of the human nervous system. The 47 reproductions are from the original works of Retzius, Cajal, Golgi, Dejerine, Kölliker, Starr, Meynert, Hammerberg, Flechsig, Ranke and His; and the 79 original photo-micrographs are mostly from original preparations made in Professor His' Anatomical Institute in Leipzig, where a duplicate set of these slides are in use.

The second study, 'On the Psychology of Advertising,' gives the results of a two years' series of experiments in exposing in the dark room regular magazine advertising pages and artificially prepared ones to test the initial attention value of text matter as against cuts, of rele-

vancy and irrelevancy of each to the article advertised, of the effects of repetition on each of these four elements, of different colors, of different sizes of type and of different parts of the page. By means of answers to four sets of artificial advertisements, in which each article was presented in six different kinds of advertisements, an effort was made to get at the reasons which influence buyers in their choices. Any differences in the sexes were kept in view throughout. From the three figures and twenty tables, mostly taken on college juniors and seniors, there resulted the following conclusions:

1. Attention: (*a*) Best gained on the whole by relevant words or text matter, next by relevant and then irrelevant cuts, and least by irrelevant words.

(*b*) Through five repetitions the relevant words increased steadily in value, while the cuts decreased, and the irrelevant words remained on their initially low level.

(*c*) The female observers were attracted proportionally more than the males by cuts and by irrelevancy.

(*d*) In colors, males were attracted mostly by black and green, females by red and green.

(*e*) A progressive increase of attention within the sizes of type from 2 to 5 mm. high.

(*f*) The left side of the page was seen more than the right.

(*g*) The horizontal quarter above the middle was on the whole more effective than the other quarters, with the bottom quarter decidedly least valuable.

(*h*) Only about half our informants seemed to be influenced consciously by advertising; the females somewhat more so than the males, however. But a considerable amount of advertising acts upon people below the threshold of their conscious attention and memories.

2. The reasons given for choosing one advertisement over others as the place from which to buy were: (*a*) Decidedly foremost from the age or reliability of the firm; then from cheapness, statement of qualities, with statement of prices and style least effective.

(*b*) For last choice cheapness was by far the main reason, either directly or under the brand of 'fake,' while an unanalyzable distrust and suspicion from too great claims or details was next operative.

In 'The Vocabularies of Three Children of one Family to Two and a half Years of Age' are given in chronological order for each part of speech, the words for which our three children used some definitely associated sound. A detailed comparison of the method here used with the other published records is made to give some judgment on the wide

variations reported in the vocabularies at about two years. A detailed record is given of the different words and the total words used on a single day in each month. The bearing is then suggested of the evidence of these usually greatly underestimated vocabularies of children on the philological legends of an adult's vocabulary. A special comparison is made of the community as against the diversity in the vocabularies used by three children in the same family and surroundings. From these vocabularies, occupying 23 pages, and from the accompanying 8 tables, the following summary is made:

1. By a comparison of the methods and results in taking the vocabularies of our three children with others it appears that most of the published summaries have been greatly underestimated.

2. Owing to the weakness and illusions of memory the most trustworthy method to take a child's vocabulary is to record the new words as heard each day and then periodically to take a whole day's talk to check off with the chronological list.

3. The vocabulary of an average two-year-old child thus appears to be about 700 words; and in the next six months it doubles to about 1,400 at two and a half years.

4. A child uses from 50 per cent. to 65 per cent. of its entire vocabulary on any one day.

5. A child's vocabulary being so far larger than the popular guess or attempted estimate,—this makes one decidedly doubt the ridiculously small guesses of the philologists as to the vocabulary of adults.

6. The total talk of a child for a day—from 5,000 to 10,000 words—the number of repetitions of its most-used words, or even a verbatim report of its talk for a half-hour, shows its immense activity and its real education through this activity.

7. Children in the same family and environment show surprising individual differences in the vocabularies used; two such children having less than half their vocabularies in common and over a quarter of them exclusively.

8. The words making up a vocabulary, the order of its acquisition and its community or diversity with other children's seem chiefly accounted for by the pleasure-pain element attached to the hearing knowledge of the word. So that, if there is sufficient interest in anything to use its word which has been heard, the motor connections having been subconsciously made, some sound is used for the word without practice.

The study on 'Taste and Smell in Articles of Diet,' compiled and tested by R. W. Tallman, goes through a large number of foods,

spices, flavoring extracts, liquors and tobaccos to separate the knowledge coming through taste from that through smell. The conclusions are as follows:

1. Having eliminated sight and smell, together with the attendant suggestion, and having deducted the touch elements, there remain usually the four fundamental tastes of sweet, sour, salt and bitter.
2. The power of perceiving these four fundamental taste sensations is not the same. Sweet is most easily and invariably recognized under all and every condition. Sour comes next and is closely followed by salt, which was occasionally not detected in peculiar, unexpected combinations. The experiments with bitter were not as extensive as with the other three tastes, but it was evident that bitter was not very readily recognized.
3. Individuals differ in their thresholds to each of the four tastes; what was quite sweet to one was not sweet to another. In sour the difference was not so marked, but existed, as it did also in salt and bitter.
4. Our perceptions of the taste of the various things we eat are greatly influenced by suggestion. It is remarkable how much one will honestly claim he tastes or smells, when, as a matter of fact, it is all due to suggested associations.
5. The sense of taste is greatly influenced by touch.
6. But the most important, conclusive and interesting result was the clear evidence obtained that almost all our so-called 'tastes,' or properly flavors, are due to smell. We have thus been acquiring a considerable part of our knowledge and pleasure-pain in a very different way and by means of a very different sense than we supposed. This is worth pondering upon and suggests an interesting inquiry as to whether this is not likely to be the case in other lines of knowledge and feeling.

In a postscript by H. Gale, the four tastes are compared with the chemical constituents of our foods, and a genetic explanation suggested, with some evidence from children, of the perception of difference in tastes and of the pleasure-pain therewith connected.

The study on 'A Case of Alleged Loss of Personal Identity' is that of an Englishman about 50 years of age, who appeared in Mankato, Minn., in October, 1896, and with very few memories of his past life. Although these lost memories gradually returned through a point of attachment and association he recalled nothing in connection with his frauds in a large lumber business in which he had enlisted Chicago capital and the collapse of which four months before had been followed by his loss of memories. In some dramatic interviews with his former Chicago partner he showed no memory or sign of guilt. An effort was made by suggestion in hypnotism to recover

these lost memories, but in vain. It was found however that he had had a similar mental lapse of shorter duration a year before, during which he was at Ward's Island Hospital, New York, and no dishonesties were connected with this first lapse of memories. In a long series of business schemes it was found that he had been an indefatigable worker, of temperate and thrifty habits, and there were many testimonials to his high personal character and to his activity in the Episcopal Church at various places. Naturally, however, the off-hand judgment on the case was that his loss of memories had been feigned to cover his dishonesties. The study concludes, however:

"When one now looks back at this strange mental composition to analyze and classify it there appear to me two important characteristics connecting him over toward the insane.

"1. A delusion of grandeur ('Groessewahn') as shown in his restless activity and pleasure in these series of large schemes started on such small foundations. His absorption in them was intense, leaving him no interest for other things except his church. In my interviews with him he could not get away from talk about himself; some brain micro-photographs, which I, for this purpose, showed and explained to him, had no interest. In his church even he pushed himself forward everywhere in an unusually commanding way.

"2. The common delusion of persecution was an easier explanation of his series of failures than the reasonable and evident lack of foresight, capital and honesty. But no such reasoning would affect him. The reversed judgment of his friends, who came to know him thoroughly, was explained too in this way.

"With these two characteristics, which are only exaggerations of the normal mental components of pleasure in activity and success and of suspicion, is specially connected the question of his consciousness of and responsibility for his dishonest practices, however great or small they really have been. That he is not a deliberate or conscious knave, or even deceiver, seems to me reasonably sure. And as therefore the ordinary motives which influence the average mental complex do not come into his consciousness with inhibitory or motor force, he is not responsible as is the average man. His case is of much interest as an example of the many components of knowledge, feeling and motor reaction, which constitute the manifold complex 'mind,' and the limitless variety of these combinations, with the wide transition region between sanity and insanity. In its non-responsibility the case illustrates the modern 'necessity' of willing as a reign of law in mental life as against the old metaphysical 'freedom.' His would be a case for restraint and not punishment.

"As to the genuineness of his loss of personal identity, it seems to me in some degree at least genuine, and perhaps entirely so. His unstable nervous temperament, with its excessive activity and disposition to worry and suspicion, was favorable to such a catastrophe. For the first collapse in August, 1895, though only of short duration, there seems no motive for deception except to gain a retreat and rest. And if the first one was genuine, the still greater stress of circumstances which culminated in June, 1896, was enough to cause a greater mental rupture. However, the suspicious limits of his recovered memories remind one of the often wonderful ingenious reasoning and cunning self-control of the insane, and give much evidence to the probability that with some real loss of memories he consciously or unconsciously—as it seems to me more likely—used his more or less mental break to cover his dishonesties."

The last article on 'Psychical Research in American Universities' gives a report of any work done in our universities in the group of subjects vaguely embraced under psychical research and of the judgment or attitude of American psychologists toward this work. A plea is made for its usefulness in an education of culture; and for convenience of use there is appended a note of references to psychical research phenomena, aside from telepathy, in the publications of the Society for Psychical Research.

Thus, as the preface explains, these Studies are largely of a personal and popular character. But it is hoped that this character may not prevent any scientific work or value there may be in the Studies from contributing somewhat to the progress of our knowledge in this exhaustless psychological field.

AUTHOR'S ABSTRACT.

NEUROLOGICAL.

Theoretisches zur Tierpsychologie und vergleichenden Neurophysiologie. Prof. Dr. HEINRICH ERNST ZIEGLER. 'Biologisches Centralblatt,' Band XX., Nr. 1. 1 January, 1900. Pp. 1-16.

After entering a protest against according reflexes and instinct to animals so low in development as to be without (obvious) nervous systems, and the brief discussion of certain biological terms, the author, a zoölogist at Heidelberg, suggests the manner in which the neurons may develop with mental and psychophysical acquirement and incidentally, as it were, advances the terminology of neurology in a useful way. He accepts as valid the claims of Bethe, Apáthy,

Schultze, Nissl, etc., as to the 'primitive fibrillæ' extending from cell to cell.

Bethe, Beer and Uexküll have termed 'antikinesis' that nervous energy traversing the reflex arc, calling it a 'reflex' when it always travels the same path, but such currents as are modified by previous occurrences, thus going differently, they term 'antiklisis.' Of the first variety are the inherited coördinations and associations which in the individuals of a species are practically the same: reflexes and instincts. These Dr. Ziegler would have called '*kleronomic*' or inherited, paths, while such paths as are opened in the neural maze by the acquirements and experience of the individual he would term *enbiontic*—those associations and coördinations which have been learned and which are not, as Weismann seems to have shown, inheritable.

"The cells of the neural apparatus are joined together by the end-twigs of the branches in the neuropile and continue as the fibrillæ which extend within the cell-body from the end-twigg of a cell to another end-twigg of the cell," that is, in fine, are association-fibrils within the cell as well as intercellular, as Bethe and others have shown.

As to the origin of the *kleronomic* (inherited) and *enbiontic* (acquired) paths, Professor Ziegler has this to say: It is no more difficult to conceive how the former are produced than to see how any paths of the ontogeny are made. The latter, *enbiontic*, paths are not so easily explained. "Think of a complex of cells which are bound together in various ways by the end-twigs of the branches and through whose cell-bodies a great multitude of neurofibrils pass; these fibrils may be present already as weak and very fine structures and extending in many directions make all possible combinations, so to say, of connections between the branches; in case a network of fine fibrillæ exists it is the same. But in the case of the central organs where numerous sense organs are in connection receiving different sorts of stimuli, continual agreement of the stimuli from the outer world will stimulate single paths. This agitation as a functional stimulus produces a strengthening of the path related to it, thickening certain numerous paths through the fine neurofibrils, as also certain of the end-twigs. This in particular will be the case when the same combination of stimuli is frequently repeated, as also whenever the same object or occurrence is frequently experienced."

Wernicke, Mann, Biedermann and Bethe, the author admits, have approached this same view, but have never made it explicit. The article is illustrated by drawings showing the neurons as they appear in the various stages described.

Meragia Paræsthetica. J. H. MUSSER, M.D., and JOSEPH SAILER, M.D. *Journal of Nervous and Mental Disease*, Vol. 27, No. 1, January, 1900. Pp. 16-40.

Meralgia paræsthetica is the name given by Roth to a newly described 'neurosis of a nerve,' namely the external cutaneous nerve of the human thigh. This article gives a general description of the 'disease,' and adds account of ten cases to the three or four score already reported. The condition is of interest to the psychologist especially because it provides a striking illustration of the large number of sensational variations which may be correlated with disturbance, functional or structural, or both, in a single small nerve. The writers of this article define the 'neurosis' as 'a disturbance of sensation on the external surface of the thigh, characterized by various forms of paræsthesia, associated with dissociation and more or less diminution of sensation.' Its etiology and pathology are as yet probably unknown, while cure has not as yet been accomplished. It seems more similar to 'neuralgia' than to anything else, but why it should be confined to one limited portion of the external nerves is quite mysterious.

Among the subjective manifestations of the affection described by various observers may be mentioned these: pain of very many sorts; hyperæsthesia to touch and to pressure; hypæsthesia, or lessening of sensations of touch, pain and temperature, and of power of localization; insensitivity to electricity; numbness and prickling; burning sensation, coldness, delayed sensory perception, feeling as if flesh were loose, sensation of moisture.

The condition seems to be associated with a lowering of the general bodily tone, but is not generally more common in hysterics than in those of less neurotic habit. This is a suggestive example of what may be correlated in consciousness with wholly invisible changes in a portion of a small nerve or in its central organs.

GEORGE V. N. DEARBORN.

TUFTS COLLEGE, MEDICAL SCHOOL.

NEW BOOKS.

Foundations of Knowledge. ALEXANDER THOMAS ORMOND. London and New York, The Macmillan Co. 1900. Pp. xxvii + 528.

Tafeln zur Geschichte der Philosophie. C. STUMPF. Berlin, Speyer & Peters. 1900.

Ueber Psychologie der individuellen Differenzen. W. STERN. Leipzig, Barth. 1900.

The Life, Unpublished Letters and Philosophical Regimen of Anthony, Earl of Shaftesbury. Edited by BENJAMIN RAND. London, Swan Sonnenschein & Co.; New York, The Macmillan Co. 1900. Pp. xxx + 535.

NOTES.

WE record with deep regret the death of Dr. Henry Sidgwick, Knightbridge professor of moral philosophy at Cambridge, which occurred on August 28th.

PROFESSOR THOMAS DAVIDSON, well known for his contributions to philosophy and education, died on September 19th, at the age of sixty years.

IN the death of Dr. Harold Griffing psychology loses a student of much ability and promise. Dr. Griffing's inaugural thesis at Columbia University, 'On Sensations of Pressure and Impact,' was issued as the first Monograph Supplement of the *PSYCHOLOGICAL REVIEW*, and he was the author of other valuable contributions to psychology, published in this *REVIEW*, in the *American Journal of Psychology* and in the *Philosophical Review*. It deserves to be put on record that Dr. Griffing had dependent on him for support a mother and an invalid sister, and was consequently unable to accept such university positions with small salary as were open to him. He was obliged to undertake the practice of law, but it was his intention to return to work in psychology when circumstances permitted.

PROFESSOR W. JAMES is still abroad but may return soon to Cambridge. His health has improved.

PROFESSOR GEORGE T. LADD has returned to New Haven after a year's absence spent chiefly in Japan and India, where by special invitation he delivered lectures on philosophy and education at a number of the leading universities.

PROFESSOR J. G. HIBBEN, of Princeton University, is spending the year abroad and is at present in Strasburg.

PROFESSOR ROBERTS LATTA, lecturer in logic and philosophy in the University of St. Andrews, has been appointed to the chair of moral philosophy in the University of Aberdeen, vacant by the transfer of Professor Sorley to the corresponding chair at Cambridge University.

RICHARD K. PIEZ, Pd.D. (New York University), has been appointed professor of psychology at the State Normal School, Oswego, N. Y. Dr. Piez assumed the duties of his chair upon his recent return from a special tour in Europe, in which he made a study of the applications of modern pedagogy in the actual work of continental schools. PITT. P. COLGROVE, Pd.D. (1900), has resumed his duties at the State Normal School, St. Cloud, Minn., after leave of absence extending over two years, which he spent in study at New York University. Dr. Colgrove will have charge of the departments of psychology and mathematics.

A *Revue de philosophie*, under Catholic auspices, will be published bi-monthly at Paris, beginning November 1st. It will be edited by M. R. P. Peillaube.

PROFESSOR E. C. SANFORD'S Course in Experimental Psychology has been translated into French by Dr. A. Schinz.

PROFESSOR W. JAMES asks us to state that the appearance of his name as member for America of the Council of Organization of the 'Institute Psychique International' is unauthorized.

In printing Dunlap's 'Effect of Imperceptible Shadows on the Judgment of Distance,' in the last number of this REVIEW, an error occurred in designating several of the curves. The two curves on p. 449, marked Figs. 7 and 8, belong to the group on p. 451 and should be designated Figs. 11 and 12, respectively; and the two printed as Figs. 11 and 12 should be carried over to the group on p. 449 and be marked Figs. 7 and 8. In the same 'studies' the printers insisted on using 'visible' in place of 'visibile,' although the error was corrected in the proof.

ALL communications for the editors of the PSYCHOLOGICAL REVIEW should be sent during the year beginning November 1, 1900, to Professor J. Mark Baldwin, Princeton, N. J.

INDEX OF SUBJECTS.

- Aberglaube und Zauberei, Lehmann, 178
 Acoustics, 185
 Advertising, Psychology of, 631
 Aesthetics, 580, 618
 After-images, 39, 63, 85, 621
 Analytical, 296
 Année psychologique, Binet's, 192
 Apperception, 145
 Arithmetical Prodigy, 135
 Atomism, psychological, 1
 Attention, 315
 Auditory Space, 139
- Character, Making of, MacCunn's, 517
 Child, Physical; Nature of, 97
 Children, School, Moral Perceptions of, 135; Individual Tests of, 135, 214; Fatigue in, 136; Vocabularies of, 633
 Choice, 149
 Chronoscope, Pendulum, 483
 Color, Changes of the White Light After-image, 39; Vision, 300, 518; Blindness, 518; Perception and Preference, 521
 Colors, Pleasantness of, 87
 Comparative Psychology, 74, 201, 424, 636
 Conjuring Deceptions, Psychology of, 617
 Conscious Complexes, 377
 Consciousness, 149, 298
- Diffusion of the Motor Impulse, 29
 Drugs, Influence on Perception and Retention, 625
- Educational Psychology, 97
 Emotion, Professor Stumpf on, 54
 Ethics, 98; and Religion, 152
 Experimental, 420, 625
- Fatigue, 466, 529, 547
 Feeling, 94
- Green, T. H., Philosophy of, 150
 Group Perception, 58
- Habit, 315
 Hearing, 88, 623
- Identity, Personal, Loss of, 634
 Illusion, of Deflected Threads, 490, 606; of Length, 592
 Images, Shadow Images on the, 18; After, 39, 63, 85, 621
 Impulse, Motor, Diffusion of the, 29
 Indes, Des, à la Planète Mars, Flournoy's, 406
 Inference, 147
 Instinct, 201
- Judgment, 152, 435; of Direction of Lines, 142; of Magnitude, 344
- Laboratory Studies, Clark, 142; Wisconsin, 143; Cornell, 314; California, 429; Minnesota, 630
 Lapses, 312
 Length, Illusion of, 592
 Localization, 429
 Logic and Philosophy, 214
 Love, Primitive, 411
- Magnet Registration Key, 612
 Magnitude, Judgments of, 344
 Melody, 241, 400
 Memory, 200, 305, 522
 Mental Imagery, 314; Fatigue, 466, 547
 Meralgia Paræsthetica, 638
 Metaphysical Method, 146
 Minimum Visible, 429
 Monadologie, La nouvelle, Renouvier and Prat's, 291
 Motor, Impulse, Diffusion of the, 29; Image, 102; Diffusion, 139; Centers, 321; and Sensory, 390
 Music, 138, 185
- Natural History Point of View in Psychology, 143
 Nature and Choice, 149
 Nerve, cell, 320, 422, 529; Endings, Motor and Sensory, 423
 Nervous System, 206, 670
 Neurology, 102, 206, 320, 422, 636
 Newspaper Science, 64
 Number Form, A New, 137
- Order, Natural and Moral, of the Universe, 151
 Orientation and Hearing, 623

- Pathology, 529
 Pendulum Chronoscope, 483
 Perception, 296; Group, Genesis of
 General Ideas from, 58; Visual, 195,
 197, 454; and Retention, 524; In-
 fluence of Drugs on, 625
 Perceptions, Moral, 135
 Philosophy and Logic, 214
 Phonetics, Experimental, 198
 Physical Psychology, 172
 Physics, Philosophy, Psychology and,
 100
 Pleasantness of Colors, 87
 Pneumatic Shutter, 281
 Pseudoscope, 47
 Psychological, Atomism, 1; Associa-
 tion, American, 125; Theory of Mu-
 sic, 138; Theory of Memory, 241;
 Æsthetics, 580, 618
 Psychology, Stout's Manual of, 66;
 Comparative, 74, 201, 424, 636; and
 Education, 97, 98; and Social Prac-
 tice, 105, 127; How Should it be
 Taught, 128; Natural History Point
 of View in, 143; Physical, 172; Sci-
 entific, 209; of Religion, Starbuck's,
 509; Fourth International Congress,
 533; of Advertising, 631; of Conjur-
 ing, Deceptions, 617
 Psychotherapy, 210

 Reaction-time, 526
 Reading, 188, 414
 Reality, Ladd's Theory of, 70
 Religion, and Ethics, 152; Psychol-
 ogy of, Starbuck's, 509
 Retention and Perception, 524
 Retina, Shadow Images on the, 18;
 Dissimilarity in Function of the Rods
 and Cones of the, 600
 Rhythm, 309, 417, 627

 Sensation, 134; Criterion of, 159; Der-
 mal, 530, Analysis of, 619
 Shadow Images on the Retina, 18
 Shadows, Imperceptible, and Judg-
 ments of Distance, 435
 Sense Types, 137
 'Sensory' and 'Motor,' 390
 Sentiments, Psychologie des, Rauh's
 182
 Social, Practice and Psychology, 105,
 127; and Ethical Interpretations,
 Baldwin's, 295
 Sociology, 211
 Somnambulism, 406
 Spinoza, Doctrine of, 289
 Spiritual Life, Coe on the, 615
 Stroboscopic Phenomena, 412
 Substance, Concept of, 147
 Synæsthesia, 419

 Taste, 91, 633
 Threshold, Space, for Colors, 300, 610
 Time and Space in Vision, 141; 325
 Tones, Duration of, 88
 Touch, 92
 Training, Special, Influence on Gen-
 eral Ability, 140

 Vibrations, Voice, 138
 Vision, 412; Time and Space in, 141,
 325; with the Moving Eye, 507
 Visual, Perception, 195, 197, 454; Im-
 pressions, Relation of Stimulus to
 Sensation, 217
 Volition, 133, 494

 Writing, 14, 20

 Zöllner Diagram, 356

INDEX OF NAMES.

Names of contributors are printed in SMALL CAPITALS, and the page numbers of the contributions in **Full Face Type**. In the case of authors reviewed the page numbers are in *Italics* and in the case of mention in the notes they are in Roman type.

- Abraham, O., 88
 Ach, N., 625
 Aikens, H. A., 131
 Alechsieff, N., 526
 ALLIN, A., 417
 Andriezen, W. L., 209
 ANGELL, J. R., 522

 Bakewell, C. M., 428
 Baldwin, J. M., 295, 427, 532
 Barker, L. F., 206
 BAWDEN, H. H., 74, 312, 390, 420
 Bechterew, W. v., 103, 321
 Bentley, I. M., 200
 Bergström, J. A., 483, 526, 612
 Berr, H., 214
 Bethe, A., 201
 Binet, A., 192
 Boeri, G., 529
 Bolton, T. L., 136, 323
 Bryan, W. L., 135
 BUCHNER, E. F., 94, 133, 137, 494
 Buecher, K., 417
 Burguet, A. Z., 198

 Caird, E., 323
 Caldwell, W., 146
 CALKINS, M. W., 149, 298, 377, 580
 CATTELL, J. McK., 100, 141, 325, 412, 507
 Coe, George A., 615
 Cohn, J., 87
 Colgrove, P. P.,
 Cornelius, H., 298
 Creighton, J. E., 150
 Cyon, E. von., 623

 Davidson, T., 639
 DEARBORN, G. V. N., 102, 206, 209, 320, 422, 424, 529, 531, 623, 636
 Delabarre, E. B., 142, 427
 Denkow, Z. R. H., 305
 DEWEY, J., 105, 127
 Dexter, E. G., 206
 Diehl, A., 420
 DODGE, R., 188, 412, 622
 Downey, J. E., 216
 DUTTON, S. T., 517
 DUNLAP, K., 435
 Duprat, G. L., 295

 Dürr, E., 412
 Edinger, L., 202
 Erdmann, B., 188
 Ettlinger, M., 309
 Everett, W. G., 152

 Ferrière, E., 289
 Finck, H. T., 411
 Finzi, J., 524
 Flournoy, T., 406
 FRANKLIN, C. L., 300, 415, 518, 600, 621
 FRANZ, S. I., 63, 85, 188
 Frey, M. v., 92
 FULLERTON, G. S., 128, 134, 159, 289

 GALE, H., 630
 GARDINER, H. N., 54, 182, 291
 GERMANN, G. B., 315

 Golgi, C., 532
 Griffin, E. H., 143
 Griffing, H., 639
 Guerrini, G., 529

 Hartenberg, P., 102
 Heilman, K., 98
 Heinze, M., 323
 HIBBEN, J. G., 70, 147, 214, 639
 Huber, G. C., 423
 Huey, E. B., 414
 HYSLOP, J. H., 64, 145

 Irons, D., 151

 James, W., 323, 639
 Janet, P., 532
 JASTROW, J., 47, 66, 104, 130, 143, 178, 406, 617
 Jennings, H. S., 202, 424
 JONES, A. L., 618
 JUDD, C. H., 133, 138, 195, 197, 490, 606

 KAHLENBERG, L., 91
 KENNEDY, F., 305
 Kiesow, F., 92
 KIRKPATRICK, E. A., 274
 Kirschmann, A., 141
 Kries, J. v., 600

6712601-10100
NOV 1 1900

- Kuntz, W., 618
- Ladd, G. T., 70, 640
Lane, W. B., 300
Latta, R., 639
LAY, W., 200, 314, 419
Lehmann, A., 178
Leighton, J. A., 146
LEUBA, J. H., 154, 509
Lindley, E. H., 135
LLOYD, A. H., 143, 172
Loeb, J., 74
Lord, H. G., 428
Luciani, 532
- MacCunn, J., 517
MACDOUGALL, R., 138, 281, 309, 524, 625
McGilvary, E. B., 150
McLennan, S. F., 323
MAJOR, D. R., 87
Marinesco, G., 422
Martin, L. J., 104
Martinotti, S. C., 320
Meinong, A., 284, 296
Mercier, 215
Messenger, J. F., 428
Meyer, M., 88, 90, 138, 241, 400, 428
Mezes, S. E., 531
Micklejohn, A., 147
Mollien, R., 427
Monroe, W. S., 135
MORGAN, C. L., 217
Munk, H., 622
MÜNSTERBERG, H., 1
Musser, J., 638
- Nagel, W. A., 600
Normann, W. W., 202
- ORMOND, A. T., 98
- PAGE, E. A., 185
Paulsen, F., 98
Peckham, E. G., 201
Peckham, G. W., 201
Peirce, B. O., 197
Perry, R. B., 428
PIERCE, A. H., 139, 356, 428, 490, 606
Piez, R. K., 640
PILLSBURY, W. B., 428, 619
Poynting, J. H., 100
Prat, L., 291
- Rachlmann, E., 518
Rank, F., 182
Rebec, G., 428
Renouvier, C., 291
Ribert, L., 214
Ribot, T., 216
RICHARDSON, WM. W., 29
- Rowe, S. H., 97
Royce, J., 427
- Sachs, M., 195
Sailer, J., 638
SANFORD, E. C., 97, 142
Schäfer, K., 88, 90
Schinz, A., 147, 640
Schumann, F. S., 284, 619
Scott, W. D., 531
Scripture, E. W., 138, 143
Seashore, C. E., 142, 592
Secor, W. B., 314
Seelye, H. H., 210
Sidgwick, H., 639
Silvestro, R. Di, 531
SINGER, Jr., E. A., 149, 296
Small, W. S., 424
Smith, M. K., 621
Sneath, E. H., 154
SOLOMONS, L. M., 216, 234
Sorley, W. R., 531
STANLEY, H. M., 58, 284, 411
SARBUCK, E. D., 509, 615
Steffens, L., 522
Sternberg, W., 91
Stöhr, A., 415
Stout, G. F., 66
STRATTON, G. M., 429, 610
Stumpf, C., 54, 185
Stupuy, H., 427
- Talbot, E. B., 152
Tarde, G., 211, 216
TAWNEY, G. A., 92, 98
Thilly, F., 98
THORNDIKE, E. L., 139, 201, 344, 424, 466, 547
Titchener, E. B., 94
TOSTI, G., 211, 295
Triplett, N., 617
Tufts, J. H., 324
- VERHOEFF, F. H., 188, 610
Vierkandt, A., 324
Vogt, R., 315
- Walther, A., 621
WARREN, H. C., 137, 192, 211, 533
WASHBURN, M. F., 39, 63, 428
WEAD, C. K., 400
Whipple, G., 419
Whitman, C. O., 202
WILLIAMS, M. C., 592
WISSLER, C., 29, 139, 198
Wlassak, R., 195
WOODWORTH, R. S., 140, 344
Wreschner, A., 428
Wundt, W., 94, 323, 427
- Ziegler, H. E., 636
Ziehen, T., 531

